FIFRA SCIENTIFIC ADVISORY PANEL (SAP)

OPEN MEETING

SEPTEMBER 9 - 10, 2004

FUMIGANT BYSTANDER EXPOSURE MODEL REVIEW:
SOIL FUMIGANT EXPOSURE ASSESSMENT SYSTEM (SOFEA)
USING TELONE AS A CASE STUDY

THURSDAY, SEPTEMBER 9, 2004

VOLUME I OF II

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Reported by: Frances M. Freeman, Stenographer

C O N T E N T S

DR. HEERINGA: Good morning, everyone. My name is Steve Heeringa. I'm the Chair for our two-day meeting. I would like to welcome you to this meeting of the FIFRA Scientific Advisory Panel on the topic of the Fumigant Bystander Exposure Model Review, this time focusing on SOil Fumigant Exposure Assessment System, SOFEA, using Telone as a case study.

I would like to welcome everybody here for this two-day meeting. A number of you have participated in similar sessions held in late August. I would like to at this point, before we begin the general proceedings, to introduce the members of the panel. I would like to begin on my right with Dr. Stuart Handwerger.

DR. HANDWERGER: I'm Stuart Handwerger, I'm in molecular and cellular endocrinology from the Departments of Pediatrics and Cell Biology at the University of Cincinnati College of Medicine. My major interests, far removed from the topic of this meeting, is the molecular mechanisms involved in fetal growth and development.

DR. ARYA: I am Pal Arya, Professor of Meteorology at North Carolina State University. My interests are primarily in air pollution, meteorology, micrometeorology, dispersion, short-range dispersion of pollutants.

DR. SPICER: My name is Tom Spicer. I am Professor and Head of Chemical Engineering at the University of Arkansas. My research interest is in near term atmospheric dispersion.

DR. HANNA: I am Adel Hanna. I am a Research Professor at the University of North Carolina at Chapel Hill. My area of interest is air quality and meteorological modeling and analysis.

DR. MACDONALD: Peter Macdonald, Professor of Mathematics and Statistics at McMaster University in Canada, with general expertise in applied statistics.

DR. SHOKES: Fred Shokes, with Virginia Tech at the Tidewater Agricultural Research and Extension Center. I am the director there and a plant pathologist. I have a background in working with soil-borne pathogens.

DR. BARTLETT: Paul Bartlett, Queens College, City University of New York, my interest is air transport modeling, measurements, environmental fate, primarily semivolatiles.

DR. GOUVEIA: Frank Gouveia, at Lawrence Livermore Lab. I'm a meteorologist mainly focused in dispersion meteorology, sampling studies and monitoring studies.

DR. COHEN: Mark Cohen, NOAA Air Resources Laboratory, in Silver Spring. I'm an atmospheric scientist working on large-scale models. I'm trying to determine source receptor relationships. I've worked on dioxin, atrazine and, now, mercury.

DR. POTTER: Tom Potter, USDA/ARS, Southeast Watershed Laboratory in Tifton, Georgia. I'm a Research Chemist. I'm working primarily on pesticide fate and transport at watershed scales in assessing exposures to pesticide active ingredients.

DR. WINEGAR: I'm Eric Winegar, Principal in Applied Measurement Science. My field is primarily monitoring measurement of airborne pollutants, analytical chemistry and exposure assessment.

DR. OU: Li-Tse Ou, I'm a soil microbiologist with the University of Florida. My special area is biodegradation of organic chemicals, including pesticides.

DR. MAJEWSKI: I'm Mike Majewski. I'm with the US Geological Survey. I'm a Research Chemist. I have a background in measuring volatilization source terms from treated fields, and my interests are in the atmospheric transport and fate of organic contaminants.

DR. YATES: I'm Scott Yates. I'm a Soil Physicist with USDA/Agricultural Research Service in Riverside, California, where I am the Interim Research Leader of the Soil Physics and Pesticide Research Unit.

My area of interest is fate and transport of pesticides in soils and volatilization into the atmosphere.

DR. MAXWELL: Good morning, I'm Dave Maxwell with the National Park Services Air Resources Division in Denver, Colorado. My interest and background are in air quality meteorology, particularly pertaining to air quality modeling, air permitting and analyzing air quality monitoring data.

DR. HEERINGA: Thank you to the panel members. Again, I'm Steve Heeringa, University of Michigan, Institute for Social Research. I am a Biostatistician specializing in research design for population based studies.

I claim no special expertise for this session, but I will try to Chair it and make sure that the expertise that is present here is brought to bear to the problems at hand.

With that, I would like to introduce Joe Bailey who is the Designated Federal Official for the two-day meeting on the SOFEA model.

DR. BAILEY: Thank you, Dr. Heeringa. Good morning to everyone. I want to welcome you here and particularly welcome you to Washington's hot, humid weather. I hope you enjoy the few days you are here.

I am the Designated Federal Official for this particular FIFRA Scientific Advisory Panel meeting. As you know, this meeting will review the Soil Fumigant Exposure Assessment System or SOFEA, which uses Telone as a case study.

I want to thank Dr. Heeringa for agreeing to serve as Chair of this meeting. I want to thank both the members of the panel and the public for taking the time to attend the meeting today and tomorrow. We appreciate the time and effort the panel members have spent preparing for the meeting, considering everyone's busy schedule.

By way of background, the FIFRA SAP is a Federal Advisory Committee that provides independent scientific peer review and advice to the Agency on pesticides and pesticide related issues regarding the impact of proposed regulatory actions on human health and the environment.

The SAP only provides advice and recommendations to the Agency. All decisionmaking and implementation authority remains with the Agency.

As the DFO for the meeting, I serve as a liaison between the Panel and the Agency and am responsible to ensure that all provisions of the Federal Advisory committee Act are met.

As the Designated Federal Official, critical responsibility is to work with appropriate Agency officials to ensure that all appropriate ethic regulations are satisfied. And in that capacity, panel members are briefed with provisions of the federal conflict of interest laws.

Also, each participant on the Panel has filed a standard government financial disclosure report. I, along with our Deputy Ethics Officer for the Office of Prevention Pesticides and Toxic Substances and in consultation with the Office of General Counsel, have reviewed these reports to ensure that all ethic requirements are met.

Because this is a public meeting, we are allowing an opportunity for public comment. I would like to remind any public commentors who have not preregistered to make a comment with us to limit your comments to five minutes.

And if anyone does want to make public comments today, please either let me or one of the other members on the Scientific Advisory Panel staff know.

There is a public docket for this meeting. All background materials, questions posed to the Panel by the Agency and other related documents are available in that docket. Slides of today's presentations will be added to the docket shortly and should be available within the next few days.

Also, background documents are available on our website. And both the docket and website contact information should be found on copies of the agenda that are available today.

After this meeting is concluded, the SAP will prepare a report as a response to the questions posed by the Agency considering all background materials, presentations and public comments.

This report serves as meeting minutes and we anticipate that they will be completed in about eight weeks after this meeting.

Again, I wish to thank the Panel for their participation and I'm looking forward to the discussion in today's meeting. Thank you.

DR. HEERINGA: Thank you very much, Joe. At this point I would like to make a comment that there are three members who are scheduled to be part of this panel for the next two days who were not able to make it.

Steve Roberts and Ken Portier of the University of Florida are dealing with the aftermath of hurricane

Frances, no power and flooded laboratories. We also heard due to an individual emergency that Dan Baker of Shell Global Solutions will not be here.

But I think, having worked with this Panel, we have a good, broad range of overlapping expertise. While we'll miss them in these next two days, I think we can adequately address the questions that are being presented to us.

At this point, then, I would like to open the meeting by welcoming Mr. Joseph Merenda, who is the Director of the Office of Science Coordination and Policy at the EPA. Good morning, Joe.

MR. MERENDA: Good morning, Steve. It's a great pleasure on my part to be able to welcome all of you on the panel as well as the members of the public on behalf of the U.S. Environmental Protection Agency to these two days of discussions on the SOFEA model.

The Agency's commitment to using the best available science in its risk assessments is strong.

A major part of that process is this sort of open public advisory panel meeting in which we seek to get the views of experts on some of the critical issues that our programs in this case, the office of pesticide programs, are facing in carrying out our legal responsibilities under federal statutes.

I know that it is a significant investment on the part of each of you, not only to attend these meetings, but to do the preparation and then the follow-up in preparing the report. I simply want to thank you on behalf of the Agency for providing this public service and wish you well in your discussions today and tomorrow.

I will be with you for this morning. Unfortunately, for other scheduled reasons I have to step out for this afternoon, but I wish you well. Thank you.

DR. HEERINGA: Thank you very much, Joe. At this point I would also like to introduce for an introduction and some opening remarks, Dr. Randy Perfetti, who is of the Office of Pesticides Program.

DR. PERFETTI: On behalf of Jim Jones, the Director of Pesticides Program, and myself, I would like to very much welcome this panel and thank you very much for all your efforts in reviewing this model as well as the previous two models.

I want you to know that we appreciate the Scientific Advisory Panel's work with us over the last eight years with regards to the implementation of the Food Quality Protection Act.

As we all know, soil fumigants are a high benefit chemicals for American agriculture. We're looking at the risk and benefits of soil fumigants as a group to ensure we make the decisions with respect to protecting the public health and the environment.

The model presented today, as well as the ones presented at the last meetings on this topic, are intended to predict exposures to people residing or working in the vicinity of fields treated with fumigants.

After these reviews -- actually later, in early in 2005, we will publish a comparative risk assessment for soil fumigants, which is intended -- which we intend to give serious consideration to the recommendation of this and previous panels.

Our goal is to use the best science available as we move towards decisions on the regulation of these important chemicals in late 2005.

To my right are Dr. Bruce Johnson of the California Department of Pesticide Regulation and Jeff Dawson of the Office of Pesticide Programs. They will be making some opening presentation.

The California DPR -- we have calibrated extensively with California DPR on the review of all three of these models. With respect to the models, again, there have been three different models.

There was in late August a review of PERFUM, the Probabilistic Exposure and Risk model for FUMigants. There was review of the FEMS, the Fumigant Emissions Modeling System.

Today we're going to ask that the Panel consider SOFEA, the SOil Fumigant Exposure Assessment system. Again, I'm looking forward and we all are looking forward to a great deal of active discussion and a great deal of excellent advice from this panel.

Again, I want to thank you all very much. Dr. Heeringa.

DR. HEERINGA: Dr. Perfetti, thank you very much and welcome to Mr. Dawson and Dr. Johnson from California Department of Pesticide Regulation.

At this point in time, I think we will begin our introductory presentation. I believe Jeff Dawson is scheduled to give that.

MR. DAWSON: Thank you, Dr. Heeringa. I appreciate being here again.

What I would like to do over the next 15 minutes or so is just provide a primer, basically, for today's discussion. And what I will be talking about is giving a little bit more introduction and background information.

We'll talk a little bit about our current risk assessment approach. So it will be easy for you all to compare with what SOFEA gives us that is different than our current approach.

We'll briefly talk about the SOFEA system, because you are going to hear a lot more details about that system shortly after I'm finished and then just briefly introduce the charge questions that we'll be considering later today and tomorrow.

Basically, the background information will touch on the different modeling approaches we're considering, the case studies we're going to look at today, the purpose of this particular model and our goals for today.

So as Dr. Perfetti just indicated, this is our third session where we're reviewing different models to evaluate exposures from soil fumigant use. Today we're talking about the SOil Fumigant Exposure Assessment system.

I'm here with my colleague, Bruce Johnson from the California DPR. You will be hearing from three individuals from Dow AgroSciences, which is the developer of this model, after I'm done.

Those individuals are Bruce Houtman, Steve Cryer and Ian van Wesenbeeck. I hope I got that correct.

Today we're going to be illustrating how the SOFEA model works through a series of case studies. Again, it is the SOil Fumigant Exposure Assessment system. We're using 1,3-dichloropropene monitoring data as the basis. That is commonly known as Telone.

The different case studies we are going to be looking at today -- these are in the various background information, I believe the Panel received in preparation for this meeting, are considering California tree and vine uses and then various crops over six different states was another case study that was prepared.

Then we're also looking at diverse agricultural practices in five different crops. They include crops in the tree and vine, field, nursery areas, strawberries, and then post plant vine type of use.

So what is the real purpose of this model; what does it give us that will help us in comparison to our current approach? It really provides a distributional -- or could provide a distributional estimate of bystander exposure for fumigant use.

We're particularly interested in how it can assist us in characterizing exposures of risk, especially at the higher ends of exposure.

It is also going to potentially allow us to better characterize uncertainty and variability.

Our goal in the evaluation of the model is illustrated by these five points: first and foremost, the scientific validity of the methodology, how transparent this system is, the data requirements you need to actually run the system, a big one for us is how can we use this system, for example, and apply it to different areas of the country where fumigant use might differ or conditions might differ, as well as how portable is this methodology for using it with other series of soil fumigants.

As Dr. Perfetti indicated we're currently in the process of doing a comparative risk assessments for the six major soil fumigant chemicals.

So now I'll just briefly touch on what we're doing in our current approach. So it will be a good comparative basis for the rest of today and tomorrow's discussion.

Basically, what we're doing is we're using the ISC or the Industrial Source Complex short-term model. As many of you know, this is a standard tool developed by the Office of Air, and it is routinely used in their regulatory programs.

What it is -- it provides a steady state Gaussian plume approach for looking at off-site movement of airborne residues. It can consider point sources such as smoke stacks, linear sources such as emissions from roadways and area sources, for example, and this is the case we're looking at today, are treated farm fields.

It's also worth noting that Department of Pesticide Regulation is also using this model as a basis for its approach.

Basically, what we're doing in our current approach -- I'm going to walk through in the next couple slides the different major factors that we use as the basis for our assessment.

The first one is field size and geometry. We'll probably hear more about how size and shape of field affects the results. But what we're doing are fields from 1 to 40 acres in size. We're using a square field.

We are using varied atmospheric conditions with wind speeds up to 10 miles per hour and varied stability in the atmosphere from class B to D. Basically, a typical day from relatively calm to relatively unstable.

DPR, I guess in this case for their assessments for this particular chemical, used actual meteorological data in many of their cases. We also have monitoring data that reflect different application equipment and what I call control technologies or ways to reduce emissions.

The application equipment we considered in the monitoring data included drip irrigation and application by shank injection. And then ways to control emissions that were included in the data were soil rollers that compact soil after it has been treated, the use of tarps and also the use of a raised bed approach.

From these data we calculated field emission rates or flux rates. And the flux rates we determined we're using 24 hour averages, range from 8 to 91 and the units are micrograms per square meter per second emitted. And we calculated these flux rates for the various combinations of monitoring data that we had.

What we saw in the trend and data that we had were that drip irrigation was the lowest emitter when it was buried. And the highest emitter was shank injection with the flat fume type of application approach.

Then there are other conditions in ISC that are what I would call more generic in nature. We use rural conditions. We treat the source as an area source, and we use the release height of zero meters to simulate the farm field.

This slide just basically illustrates the kind of output that we get from ISC. The treated field is there on the left, our square treated field.

Basically, what we do is we indicate that the wind direction is in one direction, 100 percent of the time for the amount of time that we're calculating air concentrations downwind. And what we do is we use ISC to calculate air concentrations at different receptor points downwind. These are just examples of the different distances that we would calculate.

This is just a table of what the output of the model might look like. In your charge document that we prepared, this is just an excerpt from that example table that's in there. So in this example, what you see is a one acre square field with an emission ratio of .19, which is I believe the highest flux rate that we used.

In the second column you see the different distances downwind. And the rest of the columns there are just the air concentrations under varied meteorological conditions. And as you move across the table, you can see that as the atmosphere becomes less stable and wind speed goes up the air concentrations goes down, as you would expect.

Then what we do with these is we calculate our risk estimates, which are called margins of exposures, MOEs, and we basically divide these exposure concentrations into some threshold level that we call the ATC or human equivalent concentration.

Again, I'll briefly just touch on the SOFEA model. You will hear more about it in a few minutes.

Basically, the SOFEA model is also based on the Industrial Use Source Complex Short-term Model. It uses emissions rates based on monitoring data and historical meteorological data from various sources.

It can calculate air concentrations downwind from specific treated fields. Also, it can consider multiple applications within an airshed. You will hear more about that concept.

The critical design elements are that it can look at source locations based on land use data, for example, the concept of ag-capable land. It can also incorporate variability in emissions and atmospheric conditions. And it does allow for the evaluation of uncertainty and variability throughout the modeling process.

So just some examples of the different inputs that have been used in these case studies by the SOFEA developers. Airsheds have been used, and that is basically a region that is considered in the modeling.

Basically, what they have done is used at a minimum 9 township grids. A township is a concept that's used in California. It is a 6 by 6 mile area established through the Ag Commissioners, I believe. So they use that township grid system as the basis for their modeling.

They have used various meteorological data sources such as from the SCRAM, that's the Center for Regulatory Air Models that is operated basically through the EPA Office of Air. They have also considered CIMIS data, which is a weather network in California that is really focused in on irrigation management in California.

They have looked at varied application methods and different emission controls like we have. They have also calculated their emissions using the aerodynamic flux method, because they have direct monitoring data from their studies and they have used our pesticide root zone model as an approach for adjusting those for different conditions and different application methods. Again, we'll hear more about that.

This is just an example of how land use is incorporated or townships are incorporated into the SOFEA model. Essentially, you can see there on the graph where those might be the areas where you would pick to do your simulation.

This is an example of flux estimate from one of the monitoring studies. You can see over time that -- over the first few days after application that flux rates are higher. And as time passes, the chemical dissipates and the amount coming off the treated field is lower, out to 14 days.

And then this is just an example of the kind of output that you could get from SOFEA. This is the 9 by 9 township grid output.

The graph there on the left is the -- it shows population density. And the graph on the right shows the emissions. And you can see that your chemical's used away -- from the area where there is a high population, you can see the emissions coming off in different areas of the region that's been modeled.

So rather than read the charge questions at this point, we'll read the specific charge questions as we get to that point of the meeting. But basically, I just wanted to talk about -- we're going to focus on three different areas.

The first one is the documentation of the modeling system. The second is the overall design, the inputs required to use the system. The third is the results and how they are presented.

So thank you very much.

DR. HEERINGA: Thank you very much, Mr. Dawson.

At this point, do any of the panel members have questions, clarification for Mr. Dawson?

Not seeing any, I think we would like to begin with the main presentation for the morning. And I would like to -- maybe, Mr. Dawson, if you would be willing to introduce the presenters.

One thing I would like to ask is that I think it is very valuable with a lot of material to find a place somewhere in your presentation to stop and entertain questions and maybe I would leave that up to you to decide when that would be.

Just think conveniently maybe about two stopping points in the presentation. One of them might be at the break. But I think it helps maybe after you have gone for 20 minutes or so to have a chance for clarification questions.

But I'll leave that up to you as to when to call that break. Dr. Arya?

DR. ARYA: I think in the document given to us, it seems to me, but it was not clear from your presentation here that in your current approach you used the fixed wind speed, wind direction and stability for a 24 hour period.

Is that right?

MR. DAWSON: That's correct.

DR. ARYA: Because my understanding is that Industrial Source Complex Model, the dispersion curves are really for one hour, averaging times or even actually less than that. It might be kind of inappropriate to use that for 24 hour average.

DR. HEERINGA: I think this is a question that we'll get into in considerable depth as we address. Mr. Dawson, if you wanted to --

MR. DAWSON: Exactly, the evolution from that is exactly why we're here, to consider the ways to move ahead from that. We recognize that there are issues with the way we're doing it now.

DR. HEERINGA: Thank you very much.

MR. DAWSON: What I would like to do now is introduce three individuals from Dow AgroSciences. I hope we said that correctly. Dr. Steve Cryer, I believe, will have the lead for the presentation from Dow. Dr. Ian van Wesenbeeck and Bruce Houtman will also address certain parts of the presentation.

They have been intimately involved in conceptual design and implementation of this modeling system over several years and also have been involved in the development of the monitoring data and dealing with the regulatory issues as well.

So thank you very much. Dr. Cryer?

I stand corrected. Bruce Houtman will start the presentation. Thank you.

MR. HOUTMAN: Good morning. My name is Bruce Houtman. I am a Product Registration Manager at Dow AgroSciences for our fumigants business.

My job is very simple. I have got about five or six introductory slides, at which point I will turn the presentation over to Dr. van Wesenbeeck and Dr. Cryer who will go through the heart of the matter for SOFEA.

I did want to start with a few introductory slides to give a little bit more background on the development of SOFEA, how it has been used and frankly for what product it has been used for. As a case study, 1,3-dichloropropene you will hear a lot about today. I just want to give a little background on that particular product.

In general, soil fumigants are used widely in this country to control soilborne nematodes, soil diseases and weeds. There are a variety of soil fumigants out there. Again, the one we'll use as our case study here is 1,3-dichloropropene.

One thing that all fumigants have in common is that they are used at fairly high use rates, they are mobile, volatile, they lead to post fumigation air concentrations following the fumigation, which can lead to inhalation exposure potential for individuals that reside near these fields. We'll call these bystanders.

The difficulty or the challenge is to have a technology or build a tool which can assess these air concentrations, their air distribution both in time and space and predict exposures that might occur and accommodate both the conditions of the use of the product, as well as the weather conditions that result in the dissipation or distribution of those air concentrations and of course understanding the manner in which these products are being used.

Although recently named SOFEA, this tool has been in development since the early 1990s. In 1990, the state of California took regulatory action against 1,3-dichloropropene, I'll call it 1,3-D, which resulted in Dow, our company at the time, Dow Elanco (ph), Dow to really embark on a comprehensive program to assess field volatility of this product and then in turn develop a tool to understand the resulting distribution of air concentrations that occur following the use of this product.

This model, as with others, is based on ISCST, now version 3. When it was originally developed it was version 1 at that time. The transition and the development of SOFEA has bridged across the different versions.

SOFEA, as it is currently called, is presently being used for regulatory decisionmaking by Cal-DPR for this particular molecule. And two features of the regulatory framework for this product in the state of California referred to as permit conditions include annual township allocations for this product, which manage the amount of product used per unit time, per unit area to manage long term exposure and risk. Also, these permit conditions include requirements for buffer zones as well.

The other background associated with SOFEA, just to give you a little bit of a feature, and, again, the detail of this particular tool will be covered in great detail by both Dr. Cryer and Dr. van Wesenbeeck, SOFEA can be used to assess anywhere from single fields up to regional assessments of air concentrations.

In terms of time, the time averaging component can describe air concentrations ranging from 24 hour time weighted averages all the way to averaging periods which can include a lifetime. It accommodates field volatility inputs, which describe, of course, the source strength. It can use long-term real weather inputs, actual product use information, which includes, again, for individual fields the manner in which the product is applied to that field, but also the seasonality of use, the distribution of uses so that regional assessments of air concentrations can be described.

And of course, buffer zones or exclusion zones can be part of that input to define the air concentrations considered in the exposure assessment.

Of course this produces a distribution of air concentrations, which allows both an understanding of exposure potential averaging over periods of time and looking over distributions or averaging of air concentrations over spaces as well.

A little bit about the development of this tool starting from left to right. Again, you will see some of this information later, but the foundation really is to assess the field volatility or the atmospheric emissions of a soil fumigant following application.

By doing individual field studies, you can understand for that field at what point does what fraction of the material volatilize from the soil and become available for off-site movement.

You can take that understanding of source strength and then model what the resulting air concentrations are off site. If that's the field, you can then model given these inputs what air concentrations occur downwind.

Now, with SOFEA, you can take these individual fields, couple them with other fields over both time and space, and you can understand air concentration distributions for entire regions.

Jeff Dawson earlier described townships as being an area of land mass over which air concentrations can be understood.

Townships are six miles by six miles. What this represents is actually an area in Kern County, California with eight townships where a lot of carrot production occurs.

If you take individual fields and input based on the source strength understanding per field and permit in the assessment for uses to occur over a season or over a year, over multiple years, you can by combining sources understand the air concentration distribution over entire regions.

Now taking that one step further, it is very difficult to see, I understand, the color points represent the townships in the state of California where soil fumigants are being used.

By understanding product use density, modeling the resulting air concentrations that occur from the product uses that occur in regions, you can understand over entire areas of the state of California, in this case where soil fumigant uses occur, what the air concentrations are and in turn get some understanding of exposure and risk potential.

A little bit about 1,3-D, again, the case study involves this material. I wanted to go over some of the specific properties, physical properties, vapor pressure boiling point. Typical use rates are 150 to 250 kilograms per hectare. There are rates that are lower. There are rates that are higher. But most uses occur within that range.

The common product use scenarios, in fact the ones that will be used in the case studies, include subsoil injection at depths 12 to 24 inches with or without tarping. The product -- 1,3-dichloropropene, the trade name or product name is Telone II, that is 1,3-dichloropropene.

In addition to subsoil injection, there are formulations of 1,3-dichloropropene which are drip irrigation applied both under surface-tarped or subsurface or buried drip circumstances. 1,3-dichloropropene plus an emulsifier is the product Telone EC.

Maybe you will see Telone EC referred to as well. That is Telone II applied through drip irrigation systems.

This is another slide on 1,3-D, field volatility losses. A number of field volatility studies have been conducted for this product.

Nominally, mass loss percentages range from 25 to 40 percent. There are circumstances where they are less. There are circumstances when they are more. But again, the nominal range of mass loss percentages ranges from 25 to 40 percent over a period of, nominally, 14 days.

Depending on some circumstances, that can be shorter, that can be longer. But the emission period is generally assumed to be a 14 day period.

The U.S. Registration status, this product has been in use since 1954. It has gone through and completed the U.S. EPA re-registration process in 1998. It most recently went through some additional bystander exposure and risk refinement where the U.S. EPA reviewed the most recent data, the best available data, and made conclusions in 2003, most notably in this case for the prescription of a buffer zone of 100 feet from treated fields.

With that -- just a little bit about the risk assessment process for fumigants, which is a little bit different than the risk assessment process for other types of agricultural chemical products.

The exposure -- the two components of exposure assessment, including exposure scenario, assumptions about breathing weight, body weights, mobility is coupled with air concentration estimates. This is the output of these air dispersion models, is air concentrations estimates coupled with assumptions about exposure conditions lead to an understanding of exposure assessment, of course, coupled with toxicity permits assessment of risk.

It is inhalation driven risk assessments for fumigants primarily. Those come from air concentration estimates from air dispersion models like we're describing.

With that, I will turn things over to Dr. van Wesenbeeck, who will go into the aspects of field volatility and environmental fate research that has gone on. This might be a time for questions.

DR. HEERINGA: Very quickly, does anybody on the panel have questions for Mr. Houtman at this point. Dr. Winegar?

DR. WINEGAR: Not really a question per se. I was wondering if the presenters could stand back a little bit for those of us on this side?

(Pause.)

DR. VAN WESENBEECK: I feel a bit like I'm at a political convention and Bruce Houtman and I are the warm-up acts for the feature presentation who is going to be Steve Cryer, who is going to talk about the guts of the model. Steve really was the main person behind all the development and hard-core programming that went into it.

I'm going to talk about some other important aspects in terms of inputs to the model.

First, I'll briefly give a little overview of the phys/chem properties. Bruce touched on them briefly.

1,3-D is actually approximately a 50/50 mixture of cis/trans isomers. It has a molecular weight of 111 grams per mol. It is a liquid at ambient temperature. The cis/trans isomers do have slightly different vapor pressures and solubilities, as you can see here.

This is how a grower receives 1,3-D in the field for those who aren't familiar. They are 110 gallon pressure cylinders, it's actually known as pigs in the grower community. They have quick connectors on them to allow growers to hook them up to their equipment with minimizing any leakage of Telone.

Briefly, some of the environmental fate properties of 1,3-D. It hydrolyzes fairly quickly at 30 degrees C, about three days, a three day half-life increasing to 51 days at 10 degrees C. So in warm climates we expect fairly rapid hydrolysis.

Aerobic soil metabolism studies in the laboratory have shown half-lifes ranging from 5 to 30 days at 25 degree C with a mode of 11 days. Anaerobic soil metabolism in the lab, half-lives of 3 to 11 days at 25 C.

For 1,3-D, that does volatilize into the atmosphere. There is the reaction with hydroxyl radicals. That is a half-life ranging from 7 to 12 hours for the cis and transisomers, respectively, based on the work by Tozon (ph).

Moving on then to the field work that Dow AgroSciences has conducted since the early 90s as Bruce indicated, obviously, for any sort of off-site air dispersion modeling the flux profiles are really critical input in terms of what is volatilizing off the field under various use conditions and climatic conditions.

Dow AgroSciences has conducted eight field studies designed to calculate flux losses for 1,3-D. Four of those are in California, one in Texas, one in Wisconsin, one in Georgia and one in Florida.

We have used direct methods, so basically, the aerodynamic method to determine flux. I'll get into more detail on that in a few slides. Another direct method we have used is flux chambers, based on technology we transferred from Dr. Yates' lab in Riverside.

Most of the studies we have conducted also have off-site monitoring which we can then use to both check our flux profile by using the ISCST model or addressing that back-calculation techniques which we have done for some of our sites.

We do prefer the aerodynamic method as a direct measurement. Again, that's the one we have used or we have chosen to use for SOFEA for California conditions.

However, SOFEA could take any flux profile as a reference flux, whether it is generated by the aerodynamic method or some other direct method like a flux chamber, back-calculated flux profiles or numerically or empirically generated.

Moving on to the aerodynamic method, I'll talk about that. Then I'll also talk about the dynamic flux chamber method and then show examples of those from three field studies, two conducted in California that were used to develop the reference flux inputs for SOFEA.

One, was a drip study using Telone EC or in-line. The other was a shank injection study in Salinas. Also, I'll talk about a drip flux study conducted in Georgia also using in-line. That's an interesting comparison of the flux chamber and aerodynamic methods of developing flux profiles.

And then I'll look a little bit at a comparison of modeled and observed off-site concentrations.

Typical field instrumentation for the flux studies that we conduct typically were on an eight plus acre plot. We need to have -- or eight acres is recommended as a minimum in order to have an adequate fetch distance.

You want to allow distance for the boundary layer to develop. The rule of thumb for that is about 100 times the distance of the highest air sampler, which in our case is one and a half meters. We like to have about 150 meters of fetch distance around the sampling mass. So typically, we're at least eight acres. Often we're at 10 or more acres.

Air samples are collected at the center of the plot at heights of 15, 33, 50, 90 and 150 centimeters. We use charcoal tubes to sorb 1,3-D. The tubes have a front and back portion, which allow us to test for breakthrough.

We want to make sure we're not getting any 1,3-D breakthrough as we suck air through that sampling tube. So by having a back portion of the tube we can analyze that separately. If there is Telone in there, then we know we may have had breakthrough.

But typically we test a random selection of the high-end samples, and it is usually not an issue.

The plot will also have anemometers and thermal couples to measure temperature at 33, 50, 90 and 150 centimeters in the center of the plot as well. I'll show some pictures of that in a minute. Most of our recent studies we have placed off-site samplers at 100, 300 feet. Some of the earlier studies in California also had samplers at 800 feet.

Typically we collect three samples per day, a morning period, an afternoon period usually centered around solar noon, and then a nighttime period. So typically a 6/6/12 hour sampling interval. That's collected then from each of the on-site samplers from the center mast and also from the off-site samplers.

In some studies we have taken soil gas measurements as well. Typically more from an efficacy perspective or to look at lateral movement distribution of the Telone within the soil.

We also have a weather station on site as well, which basically is a standard weather station with tip and bucket rain gauge, windspeed direction, relative humidity, et cetera, that we use as input for ISCST modeling when we're attempting to predict off-site concentrations.

The picture of an actual field study, this is in near Douglas, Georgia. It is a melon field. The grower has bedded up these rows here and pulled a polyethylene tarp over them.

This sort of line you see here is the drip tubing underneath the tarp. This is going to be a drip irrigation application occurring here. This is the center sampling mast in the middle of our plot. You can sort of see the sampling tubes and personal pumps here, which draw air through the charcoal tubes.

This is the thermal couple stack here and the anemometer stack here, which again, are critical inputs for the aerodynamic method. This picture here shows the weather station that we have on-site as well.

This is a close-up of the flux mast again showing the various heights that we measure. You can see the personal sampling pump here that draws air through the charcoal tube through this piece of tubing, and the charcoal is at the desired height here.

Once the samples are collected, they are immediately frozen in the field. And then they are shipped frozen overnight to a laboratory for analysis.

The 1,3-D is extracted from the charcoal using a mixture of solvents and then analyzed by gas chromatography. The LOD for most of our studies has been .03 micrograms per tube and .01 micrograms per tube for LOQ.

As I mentioned earlier, we do check for breakthrough. We have also, for every one of our studies, conducted a fairly significant number of travel and storage spikes to ensure the integrity of the samples during shipping and storage.

I just wanted to touch briefly on application rate verification too. For soil dissipation studies, this has always been really critical, what is that time zero recovery. For a volatile soil fumigant, it is more difficult to go in and take soil samples when it is shanked in or dripped in as a line source.

What we do is we verify the application rate simply by taking the mass of 1,3-D applied on the field, which we do under GLB conditions with a calibrated scale, et cetera, and the surveyed area of the field.

In the case of drip studies we also take water samples from the drip tubes to verify what the 1,3-D concentration and space and time and ensure we have uniformity in the irrigation water.

Moving on to a brief overview of the aerodynamic method, this method has been studied a fair bit over the last few decades, in some cases by folks here on the SAP. And we basically followed the modified form of the Thornthwaite-Holzman equation, which is described in detail by Majewski, et al., in their 1991 paper.

This accounts for non-stable conditions via stability correction factors on the Richardson Gradient. That's based on the log-law of the wind speed profile for boundary layer development.

This is just an example of what some air concentrations look like at the center mast in a field volatility study. Here is our five heights, the 15, 30, 50, 90 and 150. You can see a logarithmic decline in concentrations. When you do the natural log of those, you get a fairly straight line. That's done for each time period then.

This is what the modified form of the Thornthwaite-Holzman equation looks like. It is a gradient method where P is the vertical pesticide flux, K is the von Karman's constant which is related to roughness or friction at the surface, delta C is the difference of average air concentrations of the analyte measured at our flux mast at two heights and delta U is the difference of the average horizontal wind speed at those heights.

These are the correction factors for unstable conditions that allow us to use that equation when we don't have -- or when conditions are not stable. I'll just flip over this, but it is in the handouts that we have if people want to look at these equations in more detail.

And this is just then the equation, ultimately, that we can program into a spreadsheet application where we dump in our final concentrations at the various sampling heights, the wind speeds, the temperatures, et cetera. Then it's a fairly straightforward spreadsheet application to come up with flux for each time period.

This is scenario number one, then, for our field studies that I'm going to talk about. This is the California Drip Flux Study. This was conducted by Jim Knuteson at Dow AgroSciences.

The study was conducted in the San Joaquin Valley of California. The product was in-line, which is a mixture of Telone or 1,3-D and chloropicrin, about two-thirds, one-third. It was applied at 19 and a half gallons per hectare to bedded soil. The beds were tarped with a Hytibar VIF film. The furrows were not tarped.

It was a 9.4 acre field. Again, samples were collected with our typical 6/6/12 hour interval. The soil type there had 1.3 to 1.5 percent organic matter. The application was made on October 2, 1998.

This is a survey map of the study site. The center mast would have been located approximately here. This is an access roadway that split the irrigation system in half. But the irrigation lines ran this way and the beds ran that way. Here are the off-site samplers of 100 and 300 feet in the four cardinal directions off the plot.

The prevailing wind direction at this site is along this axis here.

This is just a picture from the field of an in-line pig hooked up to an irrigation system. This is a nitrogen tank which is used to pad the cylinder as the 1,3-D moves out of it.

Here again is the line going through a flow meter so we can tweak the flow to get the right concentration and irrigation water that we desire and then flowing into the irrigation header.

Here is the results from that study, the bottom line, really, which is the flux profile is calculated using the aerodynamic method. On this axis here we have 1,3-D flux in units of milligrams per meter square hour, which are the units we need to input into ISCST.

This line is the cumulative mass loss here. In this study we came up to about 29 percent total mass loss over a period of three weeks that we monitored at the study.

The purple points here is the daytime flux. And the lighter pale blue points are the nighttime flux. So we have higher flux, as expected, during the daytime periods and lower at night.

The aerodynamic flux profile measured in this study was then used directly as an input into the ISCST model using the actual weather data measured with the on-site weather station to predict what the off-site concentrations were at the 100 and 300 foot receptors that I showed you in the earlier slide.

So this is just a check really of the system. We're not doing any back-calculation here at this point. We're just running the model with the flux profile that we actually measured directly at the site using the aerodynamic method and then seeing what the model is predicting.

So this is for the 100 foot southwest off-plot receptor. And generally you see the model doesn't do too bad. It underpredicts by a factor of three or so here. It missed the timing of this peak here. But generally, this is one of the better predictions that we have gotten at this site.

Here is the 300 foot receptor in the same direction. We calculated then what the -- we did a back-calculation of the flux here as well just using the method that previous presenters have used by doing linear regression at each receptor location at each time point.

And we see that there is a reasonable match here between the back- calculated flux and the aerodynamic flux predictions.

This is just a correlation then between the aerodynamic flux rate plotted on the Y axis and the back-calculated flux rate plotted on the X axis.

So the take home here really is that field experiments can be designed so that the aerodynamic flux calculations can be verified using air dispersion modeling.

We feel that back-calculation is not quite as good as the aerodynamic estimate because of the need for accurate wind data and the model hourly time step, which increases some uncertainty in the model prediction or in the back-calculation prediction.

Moving on to flux chamber measurements, and at this point we're not actually using a flux chamber generated flux profile as an input to SOFEA, but we're throwing it out here as a possibility of another direct measurement of flux that might be useful and probably would generate data in a more cost-effective manner than a full-blown aerodynamic flux field study.

The flux chamber, the dynamic flux chamber methodology has been used extensively for measuring carbon dioxide, NO2 and other gases in agriculture. It has been examined by a number of -- closed flux chambers have been also examined by a number of researches and this methodology was transferred to us by Dr. Yates' group.

I apologize for saying UC Riverside. I realize it is the USDA lab.

So although flux chambers have been extensively compared to closed chambers in model predictions there is not a lot of data out there comparing flux chambers to the aerodynamic method.

This is what our version of the flux chambers look like. They have a 40 by 40 centimeter base. They are eight centimeters tall. There is an inlet and outlet fan to draw air through the flux chamber. And that's set at 20 liters per minute. Then there is also pumps that draw air through the sampling tubes.

In this study here we were monitoring for chloropicrin and 1,3-D. We had different flow rates here for the different compounds based on the LOD we were trying to get in the tube.

The airflow, sample flow, temperature and pressure are monitored every 15 minutes by a data logger that's contained in the unit that sits on top of the flux chamber here. It is just a battery that powers everything. I'll show a picture next.

This is what the inside of the flux chamber looks like. This is a Campbell Scientific Data Logger. Here is the SKC sampling pump, this is the same type of pump we have on the center mast in the aerodynamic method studies that draws air through the sample tubes.

We can put in one day's worth of sample tubes in there. There is a solenoid valve that switches from one tube to the next every 6 and then at 12 hours. At the end of the day someone can come out, switch out that array of sample tubes and plug in a new set. Then ship them off to the lab for analysis.

The flux chamber material balance is given by this fairly straightforward equation here where Q is the airflow rate through the chamber. A is the enclosed surface area. And C in and C out are the concentrations over the interval T.

Some of the assumptions for the flux chamber method are steady state, constant airflow, uniformed gas flux or the gas flux is uniformly constant over the sampling interval, that there is a well mixed air stream. There has been fair work done by Dr. Wang and others that have studied that, the mixing of the air stream. It seems fairly uniform for this design of flux chamber. And also that diffusion flux is greater than advection flux.

This is another field picture showing how the flux chambers are placed on the field in the case of tarped bedded agriculture. This is a drip study, again, in Georgia. The irrigation lines are underneath this polyethylene tarp.

For this study we installed five flux chambers in the field. Two are in furrow areas like this. Two are on tarped beds. One was on a bare bed with the tarp removed. We were just trying to look at the effect of the tarp.

So scenario number two, then, is the Georgia drip flux study, which is where we compare the aerodynamic and flux chamber methods. Again, in-line, which was the 1,3-D chloropicrin combination.

It was applied on December 6, 1999 on a 10.4 acre field near Douglas, Georgia. It was a sandy loam soil with very low organic matter. It was a 25 gallon per acre broadcast equivalent rate. That is the rate in the bed area.

If you start accounting for the furrow, then the rate was down at about 10 gallons per acre. The field and analytical methodology was similar to the last study I described.

This is just a plot of 1,3-D flux, here again, in milligrams per meter square per hour on the vertical access. And the orange line is the aerodynamic method and the purple line is the flux chamber method.

If you integrate these curves and come up with a cumulative mass loss, you get 29.2 percent for 1,3-D for the aerodynamic method and 20.4 percent for the flux method. The other thing to note here is the wider variations from daytime to nighttime periods in the aerodynamic method. That seems to be damped out in the flux chamber method. That could be due to some insulation qualities of the flux chamber itself. So just an artifact of that but something to consider.

Similarly, for chloropicrin, we get 12.8 percent mass loss with the aerodynamic method and 14.8 percent for the flux chamber method. Again, we get wider diurnal variations showing up in the aerodynamic method.

So the take home message here is that the aerodynamic method and flux chamber methods, at least in the Georgia situation, provided fairly similar results. And they provide decent mass loss profiles for air dispersion modeling.

Again, one of the advantages of the flux chamber method is that it is easier to replicate in a cost-effective manner and would allow one to quickly and relatively inexpensively evaluate different fumigant management strategies, different types of tarp versus no tarp or drip versus non-drip, et cetera.

So the last field scenario I'm going to talk about in terms of generating flux profiles for SOFEA is the California shank study. This is one of three conducted in California in the early 90s by Jim Knuteson.

This one was in Salinas, California. It was a 10 acre field application on September 25th at a rate of 12 gallons per acre of Telone II, with off-site samplers, eight of them in the four cardinal directions, again at 100 and 800 feet. Samples, again, were collected again on a 6/6/12 hour interval for 14 days this.

This is the study that ultimately results in the flux profile that is used in the SOFEA model for California conditions for shank injections.

This is the bottom-line result of that study.

Flux here micrograms per meter squared per hour. This is the -- you can see the flux starting at about period four or five, really after period four, which is day two, peaking at about day three or four and then slowly declining over the remaining two weeks of the study. The total mass loss here when you work it out is 26 percent.

This is a summary of 1,3-D mass loss of the eight field studies that I referred to earlier that Dow AgroSciences has conducted for 1,3-D over the past decade or so.

There was an early shank study conducted in the Imperial Valley which had an 11 percent mass loss using the aerodynamic method.

This study should probably have an asterisk beside it because since it was the first study they conducted, they were unsure how long to monitor and possibly the entire flux profile wasn't collected there. They stopped monitoring after eight days.

Subsequently all these studies have had at least 14 days to 21 days of monitoring.

So this is the Salinas shank study I just talked about, 26 percent. Another shank study in the early 90s in the San Joaquin Valley, 25 percent. This is the drip study I talked about in California with the 29 percent mass loss where we also looked at chloropicrin, since that was a Telone chloropicrin mixture. We had nine percent chloropicrin mass loss.

This is the Georgia drip study that I talked about earlier. And we had 29 percent mass loss, which is obviously very consistent with the San Joaquin drip study as well. This is where we had comparative 21 percent mass loss for the flux chamber method.

In that study also, we measured chloropicrin and we had 13 and 15 percent mass loss between comparing the aerodynamic and flux chamber method.

We conducted a buried drip. This was 5 inch buried drip with no tarp study in Rio Grande City, Texas that resulted in a 46 percent mass loss according to the aerodynamic method.

And then other geographic regions, south Florida, Immokalee, this was a shank bed study. This resulted in a 40 percent mass loss. And then a cold climate prospective groundwater study we conducted in Steven's Point, Wisconsin, which was a shank application, resulted in 22 percent mass loss.

We have a fairly good range of geographical coverage of flux profiles as determined by the aerodynamic method. As Bruce pointed out earlier they range from and 20 to about 40 percent.

This is really an iteration of what I just said. I just got ahead of myself.

One of the other important model inputs for SOFEA is product use. In SOFEA, we can generate probability distributions, things like field size, application data, application rate, et cetera, and vary those according to actual measured probability distributions to get at the uncertainty.

For the California situation, we have an excellent resource there through CDMS, which is California Data Management System. They collect and collate 1,3-D use information from the California Pesticide Use Records or PUR database.

Once it is in CDMS, it is proprietary Dow

AgroScience's information. For areas outside of California, this information may be collected by growers groups or through the commercial side of the business selling the product.

And this information also includes the 1,3-D product used, whether it was Telone II or in-line, which is the 1,3-D chloropicrin mixture and also the pest and the crop type.

This is just a sample of the spreadsheet from CDMS. It comes in Excel. It is a bit of an eye chart, but basically, it contains all the information we need to generate probability density functions for the model input.

For example, the date the application was completed, the name of the township it was in, the section. Every township in California is subdivided into 36, one square mile sections.

So we're starting to collect that data more now in the last few years. Some of the earlier CDMS data doesn't have section data.

Steve will describe later within the SOFEA model we can also specify use into specific sections based on historical use data. So that will give us some more representative characterization of exposure within a township.

So it also includes, again, field size and acres, the rate that was used, total pounds of AI. It includes an application factor, which is a unique situation to California at this point. And Steve will cover that a little bit in his discussion of the model, again, the method of injection, whether it was injected at 12 inches or deeper than 18 inches and so forth and the crop and the product.

So this is an excellent source of information for building the probability distributions that we use to drive the actual agronomic practices within the SOFEA model.

I think this is where I turn it over to Steve. It might be a good break for questions.

DR. HEERINGA: Thank you, Dr. van Wesenbeeck. I guess I would like to use this opportunity for questions at this point on the material that Dr. van Wesenbeeck has presented.

DR. SHOKES: Fred Shokes. I'm just curious. You show certain losses on that. What happens to the rest of 1,3-D? What do you think is occurring with that, that you are not measuring?

DR. VAN WESENBEECK: The slide I showed with the summary of the aerobic soil metabolism studies, we do get a fairly rapid metabolism of it, an average of about 11 days or so faster in warmer soils, for example.

We also get hydrolysis in the water phase and potentially some mobility.

DR. HEERINGA: Dr. Arya.

DR. ARYA: Regarding your chamber method, some people think, of course, that putting a chamber, closed chamber, you are modifying the environment greatly. You don't have what is occurring in the open atmosphere with the changing winds and changing stability, changing turbulence.

That might be reason, of course, that you don't get that diurnal variation, strong diurnal variation you show through this aerodynamic method.

The flux chamber method sort of gives you very smooth variation of the flux and of course much lower. Some people including my colleague, Dr. Nijad (ph), at North Carolina State University, tried a dynamic chamber method where you have a stirrer which you create turbulence within the chamber.

And I believe you don't have -- you basically don't have any mixing in the chamber. Do you employ any?

DR. VAN WESENBEECK: There are vanes that split the airflow and move it across uniformly across the chamber. I'm not sure that that necessarily generates turbulence. I believe it would maintain more the laminar flow situation.

DR. ARYA: But there is another kind of chamber they call dynamic chamber where they use a stirrer to create turbulence condition within the chamber to kind of try to mimic what is in the open atmosphere, and that gives you somewhat larger diurnal variability.

You still don't simulate the full conditions of the atmosphere, it is still disturbing the conditions.

DR. VAN WESENBEECK: That is a good point. It is agreed in somewhat of an artificial system where you have a uniform airflow, not varying wind speeds, you get a damping effect because of the insulation properties of the flux chamber itself.

We actually had thermal couples inside there. We looked at the diurnal fluctuations in temperature inside the chamber versus outside. There is a lag phase and that type of thing.

But these are things that as the methodology develops maybe could be improved. But I think it still gives a relatively good comparison between application techniques or something, because you are standardizing the method.

DR. ARYA: I think your aerodynamic approach seems very good. You are using three or four different levels and resolving the gradients very well. That's, of course, more labor intensive effort, I am sure.

DR. HEERINGA: Thank you. Dr. Majewski?

DR. MAJEWSKI: In your field studies, you use 6 and 12 hour sampling periods. I think these are, well, in my opinion, these are fairly long and would tend to dampen out the effects of the atmospheric stability during the terms and in effect underestimate the actual fluxes.

Do you have any idea or have you looked at this to look at the results from shorter periods versus these long periods? How did you decide on the 6 and 12 hour sampling periods?

DR. VAN WESENBEECK: I believe that was based on early work done in the 90s. I'm not aware that we have looked at shorter time intervals to look at the effect of atmospheric stability on the aerodynamic flux estimation.

It is probably driven largely by logistics and cost. I'm open to suggestions, though, on going to a shorter time period if needed.

When we look at the comparison between the aerodynamic -- predictions of off-site concentrations using the aerodynamic flux profile using ISCST, it comes out not too bad. So I'm assuming it is a reasonable estimation of flux.

DR. HEERINGA: Dr. Yates.

DR. YATES: I have a couple of questions. First, for the chloropicrin, did you use a charcoal sampling tube or did you use some other?

DR. VAN WESENBEECK: No, it is a tube called an XAD4. I can't remember offhand what the exact material is. I can look that up for you, but it is the sort of material we use especially for chloropicrin.

DR. YATES: The other question deals with the flux chambers again. Did you leave those on the field continuously during the experiment? Because I notice you had an automatic sampling system so you didn't really have to go out into the field.

I guess what I was wondering was if you ever moved them, like if you put it in the furrow on the soil. Periodically, did you move them to a different location?

DR. VAN WESENBEECK: No, in this case we left them in place for the duration of the experiment.

We spent a fair bit of effort trying to get a good feel between the soil and the base of the flux chamber. So we didn't want to disrupt that.

DR. YATES: I know for putting them on a tarp, the issue of changing what is happening in the soil is less affected when you have a tarp because it is already affecting the soil surface anyway.

But when you put it over soil, you affect evaporation and a variety of other things. So I was curious.

You also had pressure transducers, I guess, inside the chamber?

DR. VAN WESENBEECK: Yes.

DR. YATES: Did you observe any increase in pressure?

DR. VAN WESENBEECK: There were fluctuations in pressure. We tried to -- when we initially set them out there, we balanced the inlet and outlet to try and match the pressure in the chamber with atmospheric pressure so that there is no pressure differential.

We do monitor that on the data logger as well in 15 minute intervals. You do see fluctuations as you get wind gusts and that type of thing.

DR. YATES: One other question, with the aerodynamic, I have looked at comparisons of variety of ways of estimating the flux, the aerodynamic theoretical profile shape integrated horizontal flux.

One thing I have noticed is that at night a lot of times with the aerodynamic method you will get a very near zero flux value that is not shown by the other ways of estimating the flux.

I'm wondering if that isn't due to the stability correction term where it is not -- the flux is really higher than what the aerodynamic method tells you, because the stability correction isn't matching what is really happening in the atmosphere.

That could probably also happen on the other side for unstable conditions. But I would suspect that at least at night when you see those very low values that probably the flux is higher, which, of course, would lead to a higher total emission, not greatly higher, but at least a little bit. Anyway, that's just more of a comment.

DR. VAN WESENBEECK: I know in some of the early work, Dr. Knuteson worked on this, he did compare theoretical profile shape and the aerodynamic method. I would have to go back, we can look at that data to see if that was the case, but ultimately we went with the aerodynamic method.

DR. HEERINGA: Thank you. Dr. Cohen, Dr. Macdonald and then Dr. Arya again.

DR. COHEN: One follow-up to Dr. Arya's question. With the flux chamber, did you ever investigate the effect of flow rate through the chamber on the measured fluxes, estimated fluxes? Because that would sort of get at some of this question of you are not really measuring the real -- the system.

DR. VAN WESENBEECK: We base the flow rate just on literature data, other researchers, and that seemed like the value that was used at 20 liters per minute. We didn't look at the impact of changing that.

DR. COHEN: When you showed the flux profiles that you have measured and you say you used those as inputs to the SOFEA model, do you apply those inputs as step functions, like for the six hour periods, or did you use the line that you showed between the points?

DR. VAN WESENBEECK: No, they are step functions really based on the time period. So they are input into SOFEA as 6/6/12, 6/6/12 and that's the flux during that entire period.

DR. COHEN: Then finally, I don't expect you to tell me what the information is, but what is the nature of the proprietary Dow AgroSciences information that you say is used as input to the model? Can you give us an idea of just what sorts of things?

DR. VAN WESENBEECK: What is proprietary about it is the fact that CDMS is a company we pay, and they collect it for us and put it in a nice format we can work with. But they actually obtain the data from publicly available information which is the PUR data I was referring to, the California Pesticide Use Records.

DR. COHEN: Okay. Thank you.

DR. HEERINGA: Dr. Macdonald and then Dr. Arya.

DR. ARYA: Regarding the sampling interval, I think six hour, does that imply that you also have six hour averaging or you took samples at six hour interval, but averaging time was different, one hour, or half an hour?

DR. VAN WESENBEECK: Right, that's an integrated sample for six hours. We draw air through the charcoal tube sampler for a period of 6 hours and then 6 hours and then 12 hours. And then we extract all that 1,3-D that's on the tube. Does that answer your question?

DR. ARYA: Yes. Did you use the same sampling for weights and temperatures, those were averaged for six hours too? DR. VAN WESENBEECK: Can you repeat the question, please?

DR. ARYA: Did you use the same six hour averaging time or sampling time for winds and temperatures at different levels?

DR. VAN WESENBEECK: Yes. We used the same sampling period for each of the different heights on the flux mast and, yes, throughout the course of the study.

DR. ARYA: Because as Dr. Majewski implied earlier, some of the flux profiles relations you are using like piece of M, they were developed for one hour samples, not long samples.

I think you are averaging over six hours. You are averaging conditions, highly variable stability conditions, and I don't know if these empirical functions you are using -- they seem to be somewhat kind of unconventional too, from what I have seen in micrometeorological literature. But they have been developed more for one hour sampling periods.

DR. HEERINGA: Dr. Macdonald.

DR. MACDONALD: I'm new to pesticide applications. I have a couple of naive questions. The first one, I'm quite surprised at how high these loss rates are.

Is there an effort in the industry to say get loss rates uniformly down below 10 percent or something like that?

DR. VAN WESENBEECK: I think that most manufacturers do want to put into place management practices that do minimize mass loss. We're in the process of doing that for 1,3-D by looking at drip irrigation applications and using different kinds of tarp, et cetera.

I don't think that there is a magic number industry-wide that is sort of a standard. Because ultimately it is going to be based on meeting some exposure and risk standard. But there is also the accuracy perspective.

It is to the benefit of companies and growers to try and maintain that in the soil as long as possible, of course, to get the concentration time needed to control pests.

DR. MACDONALD: Presumably, that's one thing that is going to come out of this study. Once we have an impression of the risk, incorporate that in with the cost benefit analysis of the cost of lowering the loss rates.

The other question, the formulas you have used here are all deterministic. Are those stochasticized in any way when they go into SOFEA?

DR. VAN WESENBEECK: Not directly. That is a possibility. Steve will touch on that further when he starts talking about the model. We put in the discrete values for the flux profile. For example, for Salinas, California, at 6/6/12 hour intervals. It uses those values.

Now, we do adjust it for seasonality. In the case of California, which is at this point just an arbitrary multiplication factor, for summertime applications we increase it to 40 percent. It is also varied depending on the depth of application. And there is various ways to do that as well.

But we're not stochastically varying the mass loss at every given time point that we measured if that's what you are asking. It would be possible to include that in.

DR. HEERINGA: Dr. Bartlett, Dr. Majewski and Dr. Hanna.

DR. BARTLETT: I would like to follow up on what Dr. Majewski said about 6/6 and 12 measuring periods. When you said that you compared it with back-calculation methods for emissions, was the measurements also 6/6 and 12 in the periphery?

DR. VAN WESENBEECK: Yes, they were. The off-site measurements were also made at the same sampling intervals the center mast used for their Knapp (ph) methods, so we're basically back-calculating with those off-sites measuring the same intervals using ISCST.

DR. BARTLETT: As far as your experience goes with emissions, the different studies, the studies you are presenting now, what about the variation in periods? I notice there is kind of a delay, I'm sure depending on method of application of the peak, the peaks, basically, in the diurnal period to period -- and you have variations from 20-some to 40 percent.

But I notice when you apply the SOFEA model, for California using one particular study, and so I'm just wondering about the issue of variation of emissions whether that's appropriate or not or why you are using one study to do that or not using a variety of profiles since you are varying a lot of other factors.

DR. VAN WESENBEECK: Using the model for California situations and with input from DPR, we decided the Salinas profile was the best one to use at this time.

Now ultimately, it really is to the previous question too, we could vary that stochastically if we wanted to or take other flux profiles that were generated from California.

We do have the one in Imperial, the one in San Joaquin or others that have been generated by whatever amount.

DR. BARTLETT: A follow-up. What about the variations differences due to soil moisture, soil temperature, soil type, the different factors have been found in modeling as far as -- from your studies do you have enough information to start making adjustments on those basis or -- I notice you have seasonality factors. Is there a way you could do that more continuously using temperature, other MET values?

Like at this point it seems like your inputs to the system are independent of running ISC. This leads to the question of coupling the emission modeling with a run of the MET data or done independently and then it could also be done in -- it depends on the approach of programming. There is different ways to couple that.

DR. VAN WESENBEECK: Yes, the flux experiments, of course, wherever we conducted them, California, Georgia, Wisconsin are basically one sample and time and space under those given conditions, whatever the moisture conditions, the temperature conditions were.

We haven't looked or tried to do any sort of correlation analysis yet anyways with all the different studies we have in terms of what the impact of soil moisture was or temperature or anything.

But we have done some modeling with PRZM, and Steve Cryer modified PRZM to handle the boundary condition more appropriately for estimating flux.

And that does allow you then to vary moisture and temperature and that type of thing to get a flux estimate. But that again would be a model predicted flux. But those could be potentially verified with field studies, though.

DR. BARTLETT: I just have one last clarifying question from Dr. Arya's question and your response. The MET data is hourly. When you run ISC, you are running ISC with hourly MET data, but then basically kind of six hours straight of certain emissions. But the MET data itself is running in ISC, it has hourly data and I presume time steps appropriate to what you are looking for.

DR. VAN WESENBEECK: That's correct. ISCST runs with an hourly time step and then it just uses the same flux for a six hour period and then moves to the next flux for the next six hour period and so forth.

So that's right.

DR. HEERINGA: Thank you. Dr. Majewski then Dr. Hanna and then Dr. Potter.

DR. MAJEWSKI: I have one comment and one question. The comment is on the way the aerodynamic method describes atmospheric stability.

I haven't kept up on the literature for the last several years, but I know a lot of effort has gone into describing stability correction terms during unstable conditions. And not much has gone into describing the stability correction terms during the stable conditions.

So it would be my guess that the uncertainty with the flux estimates during the stable conditions would be a lot higher than during the day during the unstable conditions.

Then my question is you did two field studies in California. One was 8 days and then the other one was for 18.

DR. VAN WESENBEECK: There were three shank studies in California. But yes, I presented two of them here, one drip, one shank.

DR. MAJEWSKI: I was looking at the graph you showed on the cumulative losses. And it looks like the loss profile for the eight-day study is significantly different than the other profiles.

I was wondering if you could comment on why you think that is.

DR. VAN WESENBEECK: I'm not sure which -- was it a --

DR. MAJEWSKI: It is the summary of field studies. It has the map of the U.S. And then it has the four plots.

DR. VAN WESENBEECK: You are talking about the bottom one?

DR. MAJEWSKI: Yes. See how -- it looks like you have most of the emissions occurring early on in the three top studies and then the eight-day study is kind of a slow increase.

DR. VAN WESENBEECK: I believe that was the first study conducted. That was the one where I was indicating that they didn't carry the study on long enough really to catch the full emission pattern.

But we would have to go back and look at whether there was a specific soil type issue there that resulted in the delay in the flux.

But basically, if the soil is really sealed well that flux profile can be delayed for several days before you hit a peak.

DR. MAJEWSKI: Right. But it just looks like the trend is very different from the loss trends of the other ones.

DR. VAN WESENBEECK: Also keep in mind that the first, let's see, the purple line is a drip study, and we tend to see the material coming out a bit faster in that case.

And in Florida here, there is a situation where it is an extremely sandy soil. It is like 98 percent sand, half a percent organic matter. So we also see fairly rapid emission there.

I'm fairly certain if we actually looked at in detail the differences would probably be explained by soil type differences between the various studies.

DR. MAJEWSKI: Thanks.

DR. HEERINGA: Dr. Hanna.

DR. HANNA: Again, my question is concerning preparing the link between emission fluxes and ISCST3.

Were there any trial to impose a kind of temporal variation of the emission profile based for an hourly basis time inputs to the ISCST3 or just used the six hour, the same value all the time?

DR. VAN WESENBEECK: We just used the same value for the entire time step that we measured in the field. So for six hours. We haven't done any sensitivity analysis on that at this point.

DR. HANNA: The other question is directed again to the aerodynamic method. You mentioned the log-law. Has that been verified with certain observations? Because the log-law for wind speed sometimes shows some inconsistencies.

It is applied as kind of algorithm to be used but not necessarily representative to what's the actual cases in the atmosphere.

DR. VAN WESENBEECK: Since we measured that, like the wind speed and temperature, the concentration, we have that all on the spreadsheet, we do an automatic linear regression on those. We can flag situations where there is deviation from that log-law. And then either not use that data point or deal with it in some other way.

DR. HANNA: Thank you.

DR. HEERINGA: Dr. Potter.

DR. POTTER: Did you measure soil moisture in any of these studies?

DR. VAN WESENBEECK: Yes.

DR. POTTER: So you had monitoring data. Would that possibly be an explanation of the difference in flux rate, say, for the California studies?

DR. VAN WESENBEECK: That's a possibility as well. Antecedent moisture content of course is going to be related to soil texture somewhat as well. That is a factor. That will affect the flux rate for sure.

DR. POTTER: A follow-up to that. Coming from the humid southeast, I know it is hard to find a few days without rain. So I'm wondering if you add rain or any other form of precipitation during your studies and, if so, how did that impact the flux measurements?

DR. VAN WESENBEECK: Yes, I know from the three that I conducted personally, for example, the Georgia drip, we did have a small amount of rain there, but just on the order of a few millimeters.

So it wasn't long enough duration over a six-hour sampling period that we noticed any real effect there on the flux.

I think if you had hurricane Frances coming through or something during the study, you would probably see a damping of flux during that period.

But most of our studies, at least the ones that I'm aware of, we didn't have significant rainfall.

DR. POTTER: I think, in general, that would be an important variable relative to estimate in flux. I obviously have an opinion in that area, and I wanted to just come out and say it. It is certainly something that needs to be taken into account in estimating flux.

DR. VAN WESENBEECK: Okay.

DR. HEERINGA: Thank you, Dr. Potter. Dr. Yates and then Dr. Spicer, Dr. Winegar and then Dr. Cohen.

DR. YATES: It is pretty clear that there are a lot of things that affect the flux of fumigants from soil. I was kind of wondering what your thinking is on whether you get lower emissions -- for example, in the introductory remark, I guess they show a slide where they have field emissions and then it says the emissions from drip buried is less than drip raised beds, less than shank injected in beds and then shank injected flat fume.

If you look at your data for California, it looks like it doesn't really matter. The emission rate is about the same for all the different method. And I have heard a lot of people say they think drip might be a way to reduce emissions.

I am wondering what are your thoughts on that.

DR. VAN WESENBEECK: I'm not sure which study, first of all, that the EPA -- or which studies the EPA based their evaluation on.

I think that as far as your question on drip, there is competing factors, I think, that typically drip is applied at the surface or just a couple of inches down. So that inherently would suggest you get more mass loss. However, usually we have a tarp over that, which is going to reduce mass loss.

So possibly it is a wash in the end, but I think we're still learning a lot about how to manage that system. The fact also that it is with the water and allows it to move down into the soil, though, could also end up reducing the potential mass loss there by keeping it in the water phase longer.

DR. YATES: But do you think that it may just change the sort of temporal behavior of the flux, and in the end when you look at cumulative losses it may be somewhat similar?

Unless of course you are in a -- this would be say given a not so reactive soil system. If the soil is highly reactive, if you can keep it in there a little bit longer, that might be enough to do it.

But for like California soils where the organic material is pretty low, I wonder if it isn't going to maybe change the characteristic of the flux but not necessarily the total emissions.

DR. VAN WESENBEECK: Certainly our data so far suggests that it is fairly similar. I mean, we have 29 percent cumulative mass loss at both the Georgia and California site, which is within the range of what we found in the shank studies as well, and probably within the error bounds around the method.

DR. HEERINGA: Dr. Spicer.

DR. SPICER: Yes, you used 6/6 and 12 hour concentration sampling times. What are the time averages of interest as far as human exposure and that sort of thing? Are they hours, 12 hours, lifetime?

DR. VAN WESENBEECK: That's going to actually be molecule specific, really. But for the case of 1,3-D in California, specifically, the endpoint has typically been at chronic exposure endpoint where we're looking at annual average concentration.

We're looking at a much larger time period really than even 6/6 or 12 hours.

DR. SPICER: You have measured concentrations at five elevations, 15, 33, 50, 90 and 150 centimeters. Why did you choose to use the 33 and 90 centimeters when determining the flux?

DR. VAN WESENBEECK: That was -- we measure five points in order to get a good estimate of the regression and determine that the log-law is working.

The 33 and 90 choice I believe, now this is before I started doing these studies, was based on just previous work in the literature and guidance from experts.

DR. SPICER: Did you look at any sensitivity as to whether the flux changed significantly depending upon the levels that you chose?

DR. VAN WESENBEECK: No. We didn't do that.

DR. SPICER: On the MET data, you indicated earlier that there were -- you found deviations from the log-law. Were those deviations, did you notice them occurring correlated with anything in particular?

DR. VAN WESENBEECK: Sometimes very high wind speed conditions, I have noticed that a few times.

But actually, it doesn't occur that often. We have very few points, really, that we get a regression, R squared less than .95 or something.

DR. SPICER: And you are talking about a log-law that takes into account the stability functions in the law itself?

DR. VAN WESENBEECK: No, this is just a plot of wind speed or concentration as a function of height.

DR. SPICER: Not including the stability functions in the wind speed profile, then?

DR. VAN WESENBEECK: No.

DR. HEERINGA: Dr. Winegar.

DR. WINEGAR: You might have already addressed this somewhat, but in following up a little bit from the previous question with regards to the aerodynamic method and periods of high stability.

I'm looking at these two plots of the aerodynamic to flux chamber comparison for 1,3-D and chloropicrin. And there is a number of zero -- it looked like pretty close to zero emissions, zero flux. I'm wondering how real zero flux really is.

Because the chamber method doesn't show as many zeros, but the aerodynamic does.
So I have to start wondering whether that zero is real. I was wondering if you could comment on how you deal with that.

DR. VAN WESENBEECK: Well, we don't deal with it per se. I mean, that's the -- they are not actually zeros. They are probably some measured value that's just very low, and we followed the methodology and that's the number we get. So we use that.

DR. WINEGAR: Do you feel that's real then?

DR. VAN WESENBEECK: I feel it is the best estimate we can get, given the field methodology we have chosen. Perhaps there are alternate methodology or different time periods, you know, at some point we could evaluate that might take into account some of those high stability time periods or something.

But given the consistency we have had with our various field studies -- and again, because for 1,3-D we are mainly interested in the annual average exposure, that whether the value is zero or 0.1 or 0.2 is probably less critical than what the peak concentrations are. Certainly it would be true from an acute exposure as well.

DR. WINEGAR: In the previous models we were focusing on a 24 hour kind of thing. Are you saying for 1,3-D we're -- the period of interest for risk is annualized and so the integrated value is more important?

DR. VAN WESENBEECK: Right. Yes, for chronic risk. Now obviously, acute risk is being evaluated too, and the model will generate 24 hour values. But again, that would be integrated over at least three time periods.

So some of that, perhaps, lack of certainty surrounding the very low flux periods would be less important from a risk perspective.

DR. HEERINGA: Mr. Dawson, did you have a comment related to this specific topic?

MR. DAWSON: Just to follow-up on this discussion and the comments Dr. Spicer raised,just for some perspective, we're interested in using this model as with the previous cases for trying to assess various durations of exposure.

So in the particular case study presented here, the historical interest has indeed been over longer periods of exposure duration as indicated.

But we're also interested in applying this methodology for shorter term, let's say, 24 hours and less, potentially, exposure durations depending upon the chemical. And even in this case we're also currently interested in looking at shorter term exposure.

So just for some clarity there from our perspective.

DR. HEERINGA: Thank you very much. Dr. Cohen, next question.

DR. COHEN: Do you have any idea how much of the 1,3-D ends up on the tarp and what happened to the tarps after they are used in the field and then what happens to the 1,3-D on the tarps? Are the tarps used again? Are they burned?

DR. VAN WESENBEECK: I know in some cropping practices in the Southeast they will run two crops through a tarp system. So they will tarp, fumigate or fumigate, tarp and then plant crop and then harvest and then plant again into the existing tarp.

As far as tarp disposal, I'm not familiar, to be honest, what the practices with that are. I don't believe we have done any measurements of residual 1,3-D concentrations on the tarp.

DR. HEERINGA: Dr. Ou.

DR. OU: I just have one question.

When you determine the 1,3-D flux loss, did you break down to a cis isomer and trans isomer and how much loss from the two individual isomers?

DR. VAN WESENBEECK: We do analyze or quantify the isomers individually. So we could estimate a separate mass loss for the cis and for the trans. But we always add them together. For exposure and risk assessments, they are added together.

We could calculate that data. I have looked at it. You do see slightly more rapid mass loss for the cis than the trans. That makes sense based on the slightly higher volatility or the vapor pressure of the cis isomer.

DR. HEERINGA: Dr. Arya and then Winegar.

DR. ARYA: Well, my comments have to do with some of the questions on the log- law and your answers. In your description of the aerodynamic method, you describe here stability corrections. And the stability corrections, you know, piece of M, piece of C, these are the dimensionless concentration gradient or dimensionless wind gradient and concentration gradients.

The fact that they are different from one implies that the wind profile and concentration profile differ from the log-law under different stability conditions. So you are actually correcting for the stability. But this also implies that log-law does not apply under all the stability. It applies strictly under neutral conditions.

DR. VAN WESENBEECK: Maybe I misunderstood the previous question. But you are right. We do apply the stability correction factors when we ultimately calculate the flux.

I understood the question as, if I just plot the raw concentration values versus height, does that follow a log-law.

DR. ARYA: You should not force a log-law. Stability is quite different from neutral. Because your correction practice implies that you are actually taking the log different from the log-law.

DR. HEERINGA: On that specific question, Dr. Spicer, and then we'll go to Dr. Winegar.

DR. SPICER: What I was trying to ask was if you use the velocity profiles that are measured, then do you use directly the log-law, or is it the log-law that has the stability functions included in it which modify the logarithmic behavior there.

That's what I was trying to ask for the velocity profile.

DR. VAN WESENBEECK: I'm going to have to check on how exactly that is calculated within the spreadsheet. We can look at it later.

DR. HEERINGA: I think that's a perfect way to approach that. If there is a question we can get back to it.

I would like to turn to Dr. Winegar for a question and then Dr. Majewski and then we're going to move to a break, I think.

DR. WINEGAR: Referring back to the discussion about the 24 hour period, I notice on the Salinas Valley flux pattern, the first four periods are pretty low, and then it spikes up. And then it shows the diurnal pattern.

I think you might have addressed this somewhat. I just wanted you to clarify why for that first day, essentially, it is such a low flux, and then it just jumps. Is that because of the timing of the application or --

It seems to be at odds with what we have seen with some of the other chemicals and some of the other application methods.

DR. VAN WESENBEECK: I think largely that's probably an indication that there was a good soil sealing conducted after the shank application was made, that the knife traces were well closed and sealed.

So it is just the time it takes for the 1,3-D to diffuse through the soil to the surface versus escaping out through the cracks.

So I think that's the result of that delay. And that time period will be different for a much higher vapor pressure compound than 1,3-D, would come out a lot faster.

DR. WINEGAR: Did I understand you correctly that you have determined that this flux profile is representative -- you've chosen to use that for different areas across California. Is that correct?

DR. VAN WESENBEECK: Yes. It was partly chosen from a regulatory perspective, because it represented the highest mass loss that we observed in the three studies in California. So there was an element of conservatism built in there.

So we had 26 percent mass loss there and 25 percent in the other study and then 11 percent.

So we picked that study. And then it gets scaled up, as I pointed out earlier, for summertime applications to 40 percent mass loss.

DR. WINEGAR: But you're referring to the overall integrated mass loss as comparing one region versus the other. When I see this other plot that shows the percent applied over time, those are quite a bit different curves, particularly at the early time periods.

So I'm wondering about this first 24 hours that we're talking about with the risk factor with these different emission rates how that comes together. I'm not quite understanding how you can apply that one profile across the board.

DR. VAN WESENBEECK: Well, as far as 24 hour exposures, the model will put out a moving average of 24 hour exposures. So in the case of 1,3-D, we would just have that peak 24 hour exposure. Then in this case, maybe, for the second or third day after application, but that's a realistic representation of the way 1,3-D typically volatilizes out of the field, is that there is a delay after application if there has been a good sealing conducted.

DR. WINEGAR: I have to wonder if the first 24 hours is not the best period to look at, then, if the peak comes a little bit later.

DR. VAN WESENBEECK: The model generates data that would allow the user to evaluate any 24 hour time period after application and pick the one that they want to use for the purpose of their risk assessment.

DR. WINEGAR: I guess that's something for EPA to decide on their procedures.

DR. HEERINGA: I would like to offer Dr. Majewski a chance for a final question before our break.

We'll obviously have a chance to come back to these issues throughout our two-day meeting. I don't want to cut anybody off, but I think I'll keep us on schedule.

DR. MAJEWSKI: I don't have a question. I just have a point of clarification on the way the aerodynamic method is used and the wind speed profiles and concentration profiles that are used.

Granted, the log-law only applies during neutral atmospheric stability conditions. And classically, the way I have used the aerodynamic method to estimate fluxes are I plot up the wind speed with height, draw the best fit line through there, take the tangents at the points that -- the measurements points at 33 and 90, which is estimating the flux through the flux plane at 50 centimeters.

So actually we're measuring the flux through the plane at 50 centimeters above the surface. So, often when you look at the resulting wind speed and air concentration plots, they are often nearly logged linear. But they don't have to be. And the way the values that are taken off that plot and used in the equation are the best fit line, draw the tangent at that point and then calculate the value there.

So I guess you don't really need to take into account the atmospheric stability corrections during those times.

DR. HEERINGA: Thank you, everyone, Dr. Van Wesenbeeck, for the presentation, the first part of this two-part presentation and the Panel for their questions.

Again, as I mentioned, we'll have ample time to revisit these issues if they are not clear. If there are any points that come up as a result of this discussion, I'm sure that Dr. van Wesenbeeck will have a chance to present that information.

So at this point in time, I would like to call for a break. I show 11:44. Let's take a 15 minute break and resume at 11 a.m.

(Thereupon, a brief break was taken.)

DR. HEERINGA: Time is available for public comment, most likely after lunch today, probably 1:30 or 2 o'clock.

If you would like to be scheduled for a public comment, please see the Designated Federal Official, Joe Bailey, to my left here.

At this point I think we would like to continue with the second part of the presentation.

I think Dr. Stephen Cryer of Dow AgroSciences is going to do the overview of the SOFEA model. Dr. Cryer.

DR. CRYER: Good morning. What you saw earlier is some of the research that goes into predicting or some of the inputs that are required by this model. But those are not unique by any means.

I just want to point out the flux that you provide is up to you. If you have got a better technique, you want to use a numerical generated flux profile, again, you can incorporate that with SOFEA.

We just felt that at this stage, when you use a numerical model, you have typically hundreds of inputs. And they all have variability associated with them so you can run it and get different results all over the map from even a deterministic model.

We felt at this time that field profiles are more relevant to real world predictions or estimates of flux. And that's why we chose to use field measured values. But again, you are not limited to using those in this modeling system.

I just want to give a quick overview of the philosophy, at least since I've been with the company, of field scale research. Really, it is a combination of a bunch of things.

You have field and laboratory studies which you heard earlier. Now we want to go out and mimic the world. We do know there are databases out there, different models that we can use.

And then, ultimately, we have to be able to present that in a certain way that people can understand. And that's where the GIS component comes in.

So really, when I say from lab to universe, that's what we do. We try to take into account all the different avenues that we have available to us and not just focus on one aspect.

So what is SOFEA? If you strip away all the fancy graphics and stuff, it is really an input file generator for the ISCST model. It is not rocket science here. We're just generating an input file for a specific numerical model.

Again, you can probably easily modify this and dump out an input file for any model that has an input file structure and an output file structure. So that framework is there. But we have added to that as making that specific for the agronomic community.

You will see that as we go through there. It is a combination of Microsoft Excel, which is what most people use as the spreadsheet. And we also use a third-party software package, Crystal Ball Pro, and that gives us the stochastic component for the various inputs parameters.

That is a very easy program to use. Some of you might have already tried that.

So, just an overview of the inputs that get fed into SOFEA. You can see those here. I say the GIS information is optional. So if you don't have that capability, you could still run this system by just using default to say everything is ag capable land. It is all flat. I have uniform population densities or whatever the case might be. I'll give an example of that a little bit later.

But the outputs, you have got the receptor grid. So you have concentrations at various points in your simulation domain. So you can plot up these air concentration curves in terms of exceedence percentile. Or you can get contour plats. You know exactly the concentration at a specific spatial location.

So it allows you to present the data in a variety of different ways.

Again, the selection of the modeling system, ISCST3, we have heard a lot about this. I'm just going to skim over this really quickly. But the bottom line it is an hourly time step model, but you can output hourly information if you want.

In our case we typically focus on 24 hour onward through the year. Subchronic values, which I'll talk about later on, that, again, is user specified. If you want like a 15 day average you can specify that and get that.

Again, ISCST3 sensitive inputs, I mean, it's pretty clear. Meteorology drives the movement under the mass and also the flux of the source coming off the field. So you really have to get those two things right.

What was talked about earlier by Dr. van

Wesenbeeck, he covered the source term and how we get an estimate of what is coming off the field so we can further propagate that in the dispersion modeling.

You want to generate a system that is specific like I mentioned to the agronomic community to present transport. Again, for those unfamiliar with Gaussian plume it is very straightforward.

In our case we have an area source of zero height. And the mass is just convected and dispersed given your meteorological conditions.

So I have kind of listed these in order of the sensitivity. Your flux profiles are big. We heard earlier about field measurements. Again, like I mentioned when I first started, you can also put numerically generated flux patterns in there if you so choose.

And this is a way you can get at the regional variability. If you say my flux profile for this location in California is not representative of this other location, then just simulate it and put it in. You can look at the results that way.

Again, it needs meteorological conditions. You need fumigant product use inputs.

Again, if you are looking at a spatial concentration dependence, you have to know where the mass is applied, you have to know how it is applied, when it is applied, et cetera, and of course some of the environmental fate properties of your molecule.

For source strength we have two flux files that we are using, one for shank and one for drip. That's what we call our reference flux files.

And here is an example of what we're using, one for drip and one for shank. These were presented earlier. Again, these were the highest field flux rates that were observed. That's why they were chosen. Again, you are not limited to choosing. You can choose whatever profile that you want.

I want to just mention you can also estimate fluxes numerically or empirically. A lot of different methods that have been done from empirical, the one dimensional modeling, the two and three dimensional modeling.

The Riverside group at USDA has some 2 and 3-D models. You can if you want to look at effects of rainfall after an application or different soil textures or soil properties, you can have that ability to use these different models and simulate those flux patterns.

These are just some of the sensitive parameters that I have found when I used the PRZM model in terms of soil properties and stuff. But some of the biggest that turned up in that analysis were the incorporation depth and a lot of the properties that were related to temperature.

So if we can adequately cover incorporation depth and somehow how the temperature dependence on flux, we can at least capture two of the most sensitive parameters from a deterministic soil transport standpoint.

And this was a graph from a publication last year where we looked at a USDA model and also a modified version of U.S. EPA model. So I modeled PRZM3 and have seen how well that compared to some of the field flux profiles that was generated from our field group. You see, they are not bad.

So again, if you want to see numerically generated data, you have that option.

On our last bullet here, you need to bridge science and regulatory constraints. A lot of regulatory scientists don't have the ability to take a three dimensional model that doesn't have a GUI interface, for example.

And you have a very unique input file structure. It is very difficult to get that input file set up to run correctly, and when it bombs you may not have the expertise to find out why it failed.

So we have to make something that is relatively comprehensive, but yet we can give it to other people who may not have a strong scientific background in numerical modeling and be able to run it and also get reasonable results.

That is really what we're after. We're trying to couple those two together. There might be better ways of doing it, but this is one of those compromising ways where we can try to keep everybody on the same page and happy.

I mentioned our references fluxes that were used. This modeling system uses those and then it scales them accordingly in this linear fashion. You see from this equation, the F R of I, that is a scale flux rate that was observed from the field studies that was divided by the application rate.

So we numerically sample from a probability density function application rate. That way you can at least account for, if you double the application, you are going to double the mass coming off the field, for example.

But it also is updated by those last two terms, those S terms. Those are based on the depth of the incorporation and the time of the year that that application went out.

California DPR, they have a two-temperature, what are called discontinuous regime for that one factor. What I show here that for a certain time window, the summer period you would scale your reference flux up by 1.6X.

That is directly to account for temperature dependence. It is going to be warmer, obviously, in the summertime. Physical, chemical properties, diffusion coefficients, Henry's law, constance, those are all strong functions of temperature.

So it makes sense that you are going to get more flux loss in hot times than other times. The sinusoidal curve, that is something that I looked at fairly recently. If you want to get a smooth distribution or a smooth function over the year, that's something you can look at.

That pretty much follows the temperature dependence of air temperatures even though this is based on a soil model.

Now I want to look at flux loss scaled by incorporation depth. Because all applications aren't made at the same depth. At times you have different practices that farmers use, for example. So you need to account for one person might inject at 12 inches. One might go 24 inches, et cetera.

So we just -- SOFEA has the capability of assuming a linear incorporation scaling or an exponential incorporation scaling where we artificially assume that if you applied everything on the surface you are going to get

100 percent volatility loss. So that point is artificial.

There was some work from the USDA Riverside lab were they actually looked at 1,3-D and the effect of incorporation depth, again on the lab scale, that's what I show on the points here. And you see, to me, they clearly don't really follow a linear scale. It is more exponential.

But what is incorporated into SOFEA you can either assume that it is exponential, decay with depth, that scaling factor or we can do what California Department of Pesticide Regulations has recommended to us.

And that was we have a field reference trial, which I have given by this magenta dot, so that the cumulative loss is scaled accordingly linearly until you get to that incorporation depth at which point it stays constant from that point forward.

If you had measured 25 percent mass loss at 18 inches, that if you incorporated any deeper you would still get 25 percent mass loss.

This is my halfway slide, but we probably don't need this now because we're right after the break. This is just to show you there is more than corn in Indiana. Once you get you get above corn line, about ten feet high, you can get up to nice mountains and stuff like this.

Hopefully nobody is from Indiana here. I'm pulling your leg. It is actually --

DR. HEERINGA: Some of us have been there, though.

DR. CRYER: You know this is not Indiana. This is what I wish Indiana looked like. This is actually a shot from Alaska.

Where do we get our meteorological information? That was pointed out by Jeff earlier in his EPA presentation. There are two sources for some of the results that we're going to present.

One is the SCRAM data, which is the EPA website. All that information has been QA, QC'd. So that is probably a good source to go to, National Weather Service data. And also in California, we have got the CIMIS data. That's where we have actually specific information for agronomic regions in California.

But ultimately, mainly what you do is generate a weather library. So that weather library can come from historical information. If you want to use a weather generation program that can give you hourly output like what ISCST3 uses, you are more than welcome.

I have used weather generator programs to generate libraries of like 500 years of yearly values, for example. But here let's say you have 15 years of historical information, 1985 to 2000, what you would do in SOFEA is assign a uniform distribution to that.

So each year has an equal probability of being selected when you run a simulation.

I did not load my little graphic here, unfortunately, but what we have here is -- I want to show you that we have a single field. You are coupling the variability of the flux loss in addition to the variability of the wind speed.

So this is actually using a numerically generated flux loss pattern coupling it with wind for 10 days, and you would see how that plume changes and diminishes in intensity given by the color, if I had loaded the file.

But it gives you an indication that you can use something like this, I think, to design a field study, especially, if you want to learn where the predominant wind directions are, where you need to put your monitoring equipment, et cetera.

I want to talk a little bit now about going to multiple source terms in an air shed or a township. We don't know the orientation or the spatial relationship between the field edge and where a person might be residing.

We know it is going to be -- downwind air concentrations are going to be driven by wind speed and direction. So you can have this person, this little girl that's on the edge of a field, and you say how big does my buffer have to be before I'm not going to have an effect on that person.

And you can run a simulation. And you can say, well, it has to be X, Y or X feet or meters. That's for a single field. In reality, you get these areas like in California or Florida where it is high, dense agriculture where everybody, all your neighbors are treating fields and stuff, you have got multiple sources you are going to have to consider.

Because now you might have an additional exposure due to these other sources that may or may not be coming on at the same time over the time frame that you are interested in.

But you really have to account for those. That's what SOFEA tries to do for you in an easy fashion.

So we're looking at now instead of using ISCST on a single field, we're looking at a whole air shed. Now you have multiple source terms that are turning off and on at different times depending on when the farmer made an application.

The source strengths are varied depending on the time of the year and how deep you incorporated it, et cetera. You can look at a single township like I have over here on the left. Now you can start looking at what are the effects when you have multiple surrounding townships.

Now you are getting a drift component, so to speak. Let's just say you are only interested in that center township that's red, you still may have an exposure effect from neighboring fields that are even outside of that township which is six miles away.

That's what SOFEA is doing for you. If you want to look at a single field, you can. But like I said earlier, a single field is not an island unto itself. You really have to account for lots of different fields that might be in the area that you are simulating.

In California, the amount of 1,3-D mass, it is limited by a township allocation. So you can only use so much mass per township. Once you exceed that limit then you can't sell or use any more 1,3-D in that particular township.

So this was to look at the effect of multiple townships between what I call zero and five and what that effect of drift from neighboring source terms might be.

So a zero township, that would be like that center red one. You have no additional sources outside that red square. Then you might have one surrounding township, so it would be that central three by three, two surrounding townships would be the four by four, et cetera.

So these are the results of this. This is what was published a few years ago. But what it shows you is that a single -- even if you look at multiple sources in a single township, you are not capturing all the potential exposure. You have to look at surrounding townships.

And there is a big jump between the zero township versus one township on all sides or a three by three. And it is less dramatic as you increase the number of bordering townships, like you think it would, typically.

When you have drift it kind of decays off in an exponential fashion. So you get to a certain point were you don't have to go to really large air sheds. That's what I kind of summarize here. You are not gaining a whole lot when you start going to like a seven by seven or nine by nine township simulation domain at the expense of -- you are really increasing your CPU time.

SOFEA has a capability to look at what I call a 23 by 23 township simulation to show you how big that is. I just give you a reference on this. The receptors are only placed in that central three by three are the orange -- the red on this graphic. But you can have source terms that are way outside that if you know where they are and you have that information.

What we see from that, it clearly indicates the surrounding townships can and do have an impact both on 24-hour concentrations and chronic concentrations. It is more dramatic for chronic, but it is still important except at the really upper high percentiles even for the 24-hour air concentration values.

This was just a slide that showed me where the top one percent of the highest air concentrations were in a particular township. This had a lot of source terms around it. But really they are close to the treated field, like you would expect obviously. Your highest air concentrations are going to be near the source terms even for chronic.

So SOFEA -- I kind of list some of the constraints here. We want a system that had to run on PCs, again, so we can give to people who have PCs and they can run it. Some of that earlier work was done on some Unix systems which are really great from a scientific and a programming standpoint.

But not everybody has access to Unix machines or super computers and stuff. So this is a system again that we compromised to make it available that most people would be able to use.

Again, it was developed to address acute all the way up through chronic air concentrations. Again, it depends on who is using it and what they want to focus on.

One of the initial reasons why this was developed was because 1,3-D had a township allocation. But you can look at some of these townships, and some of those townships are bordering oceans. They may have a mountain range through the middle and stuff.

So really we wanted to say if we're going to put or do simulations, we want to make sure that we're simulating regions that have high ag use. That is how the system was initially thought of and began the development process.

So we were fortunate enough to have some foresight to saying we want to make this versatile enough to handle various crops. Different crops have different agronomic practices, different application equipment, different times of the year depending on what crop is growing.

So the system can break crops up into five unique crop types. And these are just headers. By no means, tree and vine, if you put input parameters that are appropriate for a kumquat, then tree and vine would be a kumquat crop, for example.

But these are the crops that were high use crops for 1,3-D and for most fumigants, for that matter. For each crop you get assigned probability density functions that would mimic that agronomic practice so the application date could be a PDF for tree and vine and be a different PDF for field crops, et cetera, for all the inputs that you can change.

I list those here, field size, type of applications, drip or shank, if it has a tarp present or not, even the time the applications is made, the hour of the day an application is made.

Where do the source terms get placed? This is, again, back to that point. You don't want to get a township and artificially place it in the ocean, which is conceivable if you don't take into account land cover.

And that's what we did. That's where the GIS portion comes in. There is a lot of public domain GIS databases out there now that everybody has access to. So let's take advantage of this and use it so we can refine the simulation procedure.

So we have digital elevation, we have population density information from the 2000 Census. We also have land cover information. So from that you can generate a graphic like I have done on the right here where water is blue and I have a mountain range that's -- not the magenta, but the other purplish color, and the magenta color, that is an urban area, a city. So the ag capable land is the light yellow color.

So now we at least know where the fields should be placed if we're going to run a regional simulation. You have to put them where the fields are.

This model is what I call a spreadsheet model. It is pretty intense. If you ever are unfortunate enough to go into the code, there are lots of lines of code. Again, like I mentioned, it's conceptually very simple what we're doing.

We're generating an input file for ISCST, running it in an automated process and then summarizing the output and doing it over and over and over again.

But it is based on Microsoft Excel. The Crystal Ball component comes in. And those are some of the graphics you would get with Crystal Ball on the right in terms of assigning probability density functions where you could set any cell in Excel. Instead of making it single valued, now you can set it as a probability density function. It's a very easy thing to do.

There is a worksheet in there, I wanted to point that out, for crop cover. You can get information from GIS sources or if you just have a hard copy map in front of you, you can get a pretty good estimate on where the mountains are, where the city is and stuff.

So there is a worksheet in there. You type in a zero, one, two, three for ag land, mountains, et cetera. It can generate like this graphic on the right for you or you can get a data dump from the GIS system.

If you don't want to type in stuff by hand or don't have a GIS guru or somebody that can help you with this, there are buttons that link to macros. You can say just make everything ag capable. Make the elevation all flat. Give me all zeros for elevation, et cetera.

So here is how our sources place and now we know where the ag-capable land is. I've got basically an iterative loop like 100,000 iterations.

It will pick a southwest corner of a field randomly in ag capable land and you also would sample your PDF so it knows the field size. So given that southwest corner, given this field size, can that field fit. I can't have it overlapping the ocean, for example. So it checks it. If it can't fit in that location, it will grab another random sample, now will it fit.

It does that up to 100,000 times. That was probably overkill. I showed 50,000 and 100,000 iterations for just randomly picking an X, Y location. It pretty much covers the whole, in this case it is a township.

We also have the capability. Again, we have historical use records on a lot of fumigants that say they are used in various locations at really high uses. You might have a county, for example, that's all ag-capable, but all use is focused on a central area.

So you have the capability of saying I want to make sure that all my treated fields are concentrated in a various area. Again, we use the section base which is one mile by one mile square. And the concept of township and section is not unique to California. That's surveyed for all of the country.

California uses it to their advantage. That's how they can track things and summarize things. But you can get township overlays, GIS overlays for all of the country.

So any way, that graphic on the right shows that you would specify basically the probability of a section within a township, a receiving field. So if I specified 100 percent in section number one, all my fields would try to be placed in that section.

You can see the outcome of that. These

artificial looking squares, those are basically sections I had picked and said those are the sections that are going to receive fields. That's why you get that high dense usage.

I want to point out each one of those fields has an associated probability density function for it in terms of rate, date, timing, size of the field, et cetera. And all those are unique per the five different crops that you can specify.

It is all different colors on here, on this particular graphic represent a different type of crop.

And I can also or you can also assume random placement. This is an example. If you assume random placement in a township, they are more spread out. They are not concentrated in a specific area, your fields or source terms.

One thing we found out early on, if a user specified section waiting, he could artificially say all the fields have to go in one or two sections. If you have lots of mass that you are saying you are going to apply to this particular air shed, they may not all fit in those two sections.

So where do you put them in they don't all fit? That's what we call the spill-over algorithm. You have to put them somewhere. We typically -- in the graphic I have here, that would be like a township section. Those little squares in there are actually fields.

And then say your next iteration you say I have a large field I need to place in there somewhere like I have in the legend there, that's not going to fit. So in that case, if it doesn't fit then it will go on the spill-over algorithm.

But the next time it comes through the loop, it says okay, now I have a little field. It is still going to try to place it in this section here. And the little field will fit in this particular example, et cetera.

So that spill-over algorithm is not initiated all the time. It is only initiated when a field won't fit. So you're still trying to pack that section with as many fields as you can possible fit if that's what the ultimate goal is.

The details of the spill-over section is in one of those preparation papers that was on the website. I'm not going into the details too much here, just a kind of glossy overview.

Let's say you specified section number eight is where you want all your fields. So you want section eight filled up with fields. Then you have an equal likely probability of spilling over to surrounding sections. That's how that would work.

If all the surrounding sections filled up then it would just randomly place fields within any ag-capable land from that point forward.

Let's say you specified section 8 and 20 as both having a 50 percent probability of receiving a field. So now the spill over effect would wait -- you still would try to get your fields in section 8 and 20 first. And then it would go over to 18. Seventeen and 16 would be the next highest probability, then by the ones that are in that lighter yellow scale.

This is my only slide I have with an equation on it. Usually, when you put an equation on, you have to apologize. This is just a purely empirical -- this is an objective function.

When you look at putting fields on a discrete basis, saying I have a 5 acre field, a 15 acre field, et cetera, we have some constraints in there. The inputs that the user specifies, he would specify the crop percentage. I have 20 percent acreage of strawberries, 50 percent of tree and vine, et cetera. You have a constraint in there.

We have a -- upfront you don't know how many fields to select. That (inaudible) is unknown in here. That's just the number of fields for different, various crops. So the term on the right, that A times R, that's the application rate times the field area. That's just a mass term.

So you have the township allocation that you specify as an input and then the total mass that is applied to that township. You want that to be zero, obviously. On the other side it would be the same thing. But now it is just crop percentage. We just have areas of the various crops divided by the total area.

Then you compare that to what you specified as inputs. This had to be done just from the standpoint that we're not working in the continuous field size, we're working in just discrete sizes when you sample from a PDF.

And the reason that it is run is you put a constraint in there, I'm interested in a township cap of this much, you want to make sure that every simulation you have exactly that much.

So we run through this optimization procedure where it tries to optimize those constraints. In a way it stretches or shrinks the fields. So once it says I need eight and tree and vine fields, six strawberry fields, and those are all different sizes depending on what it sampled through the PDF, now it stretches those or shrinks those in such a way that it still optimizes your constraints, but still meets those constraints. Hopefully, that made sense.

It is constraining. You can only stretch or shrink a field by 20 percent. If you get a big field you are going to wind up with a big field. You are not stretching a little field to a big field or a big field to a little field.

Again, this is solely to meet the input parameter constraints that you as a user would supply. Another thing we needed to look at was field re-treatment. Obviously, a lot of the farmers use a product year in year out. Fumigants are typically used on high priced commodity crops. So the farmers usually can afford that.

If you are looking at chronic air concentrations or even acute concentrations they are going to occur near those treated fields. If that same field is getting treated year in year out, that's where the highest air concentrations are going to occur. And that is what you want to know.

So we had to program that functionality in there. That's a user specified input now. Through data mining, and Bruce Johnson at CDPR did some data mining on methylbromide information, he said roughly that field re-treatment was about 50 percent. So next year about 50 percent of the farmers are going to retreat with that same product.

And how that works is the first year of simulation you place your fields in ag-capable land. And then the second year of simulation if you specified 50 percent field re-treatment it would just randomly sample 50 percent of the fields from year one and would use those fields and be retreated again the following year, et cetera. And then it would do the same procedure for year two.

So you specify at 100 percent field re-treatment, then you could have the same fields getting retreated every year throughout the rest of your simulation.

Why did we choose square fields? That is where I say why not. I mean, really, what sizes and shapes should you use. We do have information about field sizes and shapes. I just wanted to point out that even if you know the field geometry you really still have to couple that with the spatial orientation.

I give an example here that these particular fields, a square and a rectangle, given the predominant wind direction they are going to yield similar results.

You go to another extreme and now you see it is very highly dependent on orientation. I was thinking in hindsight, I would probably -- to account for that, I probably should have used circular fields instead of square or rectangles. It doesn't matter which way the wind is coming. You always have the same fetch length.

I'll also say that we don't break up large fields in a series of smaller fields. Larger fields will give you higher downwind air concentrations.

The reason we don't break it up is because it is still a possibility. If you have a farmer that can afford to hire somebody to commercially apply this, they can have two or three rigs going simultaneously. So you can have a single field, large field treated in one day.

If you know that a field is broken up, then you can indirectly assume that too by instead of having a large field in your probability density function, break it up into two equal size smaller fields. So you still have that capability.

I show a validation using SOFEA against some Kern ARB, Air Resources Board in California, monitoring data. They have been monitoring soil fumigants for a number of years. We're going to look at the 24 hour air concentrations over a monitoring window, which is about two months.

I call it a pseudo validation because pseudo -- we knew where the air monitoring locations were, but we didn't know the proximity of the treated fields to those air monitoring locations. So we just used SOFEA saying let's place fields in ag-capable land and see how well we do.

Also, we didn't have the meteorological data specific for this particular, Merced County. I got it -- it was Kern County. So we used neighboring weather information from Merced County.

So the little orange arrow, that is the monitoring window of the ARB. I show the history on there. That is the application dates for that year or two years of monitoring that for 1,3-D. You can see ideally it would have been nice if California monitored the entire year because applications are made throughout the entire year.

So what I did, I just focused on that time window from two months in the summer, whatever happened to be, and that's what I ran the simulation for. So I could compare the simulated air concentrations to what ARB measured.

Also, you can break it up into crop type. Like I said, we have the capability of simulating up to five different crop types. So I looked at what the crops are the that were grown in that area.

I broke it down into -- over the monitoring window, really, it came down to like three crops. I specified three crops. I developed probability density functions for those three crops. I used those in SOFEA with the appropriate met conditions. This is what we came up with.

So this -- I'm was quite pleased when I saw this. So this is a 24-hour daily value air concentration over like a 60-day time window. I repeated this for 10 years of simulation. So that's what the magenta line is. Ten years of simulation over that same 60-day day monitoring window was what ARB did.

You see the ARB data for that monitoring time frame. Again, it is on a log linear scale, but I was quite impressed with how well we came.

When you think about it, you should get pretty close because you know how much mass is applied in the area, roughly where it is going to be applied in the ag land and the proximity to these monitoring locations.

If you didn't get pretty close, then there would be more reason for concern.

We also made this program have the capability of looking at forecasting into the future, because management practices change over time. Farmers change their practices over time. Urban sprawl takes place taking land out of potential ag production, et cetera.

In the case of soil fumigants, if methylbromide is going away, then, obviously, some fumigants are going to have to replace methylbromide. You might have a fumigant that might increase in use down the road once methylbromide goes away.

This program has the ability of what I call five loops. You can have five loops of whatever duration that you want. In this case I say let's run a 25 year simulation. Each one of these loops will have five years each.

So you can specify different inputs now for each one of these loops. So if you know that you want different management practices in loop number three, you would specify that. So it will simulate different practices until it got to loop three, and then it would specify what you want it to look at.

For example, you are taking out land because of urban sprawl or you are going to require everybody to use a certain type of tarp, for example, to cover your field. You can simulate that in one of these loops.

You can go like from present day and anticipate what the future might hold or from a regulatory standpoint you can say what if I specify these different best management practices over time what is going to be the ultimate result from that.

It is a way to use a kind of as a predictive tool to know what might be the consequence of different practices that you might propose or specify.

I'll give you an example of how you can use it with some temporal and spatial management practices. If you look at the bottom graphic, really what we'll focus on is you can actually alter how fields are treated.

Say, for example, from year one to year two, say the light colored blue is areas of high use, let's say the following year you can switch it. That can be a regulatory constraint, if you used it one year, you can't use it the following year, et cetera.

You can kind of see, like I mentioned earlier, your highest air concentrations are going to be in your high use regions. So on the magenta, your highest air concentrations are going to be within that orange dots of receptors.

So if you switch that around, say, okay, the following year we're going to make those high use regions somewhere else, over time, like over a life time of exposure, you can get something that would look like what I have here.

Looking at that top graphic that doesn't have the color there, that just shows that those receptors -- if you always went to the same high use regions year in year out, you are going to have an extremely high lifetime average for these receptors here, here and here, because they are getting hit year in, year out with a high concentration value.

If you were living there, for example, it is going to be by far the highest exposure anybody is going to have.

Let's say you break it now into two different regions of high use. Like I mentioned, you alternate from year to year, you might have higher concentrations there that first year of use. Your second year you have a drift component that is a small fraction. Then the following year again that same receptor will get a high value and then a low value.

This is a way you can do some temporal management practices, again, with the forecasting capability of SOFEA.

I have been kind of focusing on some of the guts and how SOFEA can be used. We have developed specifically for chronic exposure assessments, but we do have an acute exposure assessment also.

You specify as a user the receptor densities.

So you can make it really coarse or really fine. The more receptors you have, the longer it is going to take ISCST to run and the more memory you are going to need, et cetera. So there is probably a happy compromise.

For this example, I specified a receptor density of 600 feet. I placed my field and I say I want a field with a 100 foot buffer.

This is where we use a modified version of ISCST3, which is modified for California Department of Pesticide Regulations where any receptors over a field or within a user specified buffer would be turned off over a time window which is typically like a reentry time period, which most registrants have.

You treat a field, you have to wait seven days before you can go back on top of that field.

So in those particular cases, then those receptors aren't used. So you can specify up front in SOFEA if you were unfortunate enough to make a really coarse grid density and you specify -- let's say I want a 100 foot buffer and you are going to get results, without looking at your data specifically, you would say I have air concentrations for 100 foot buffer.

But what might happen then is what you have here, your closest receptor that's on, so to speak, might be 300 feet away.

So you might actually be getting air concentrations at 300 even though you are thinking you are getting it at 100 because you specified 100 foot buffer. Did that make sense to everybody?

The way to get around that obviously is to increase the receptor density. But at that same time when you do that, your simulation is going to take longer to run.

The alternative is what about putting like rings of receptors around. Let's put receptors around all of our fields that are like 100 foot buffer, 200 foot, like what I have in this example here. You can say that particular receptor is 300 feet from my field.

But the problem comes in when you have multiple source terms. Now, let's say you have another source that is only 100 feet away. So now you are trying to say -- so you might get like a really high air concentration at 300 feet away. It is not due to that middle source. It is due to a neighboring source that's a lot closer than you know of.

So now we have the capability. As a user, you would specify I want to look at 10 buffer setbacks. I want to make sure that if you specify 100 foot buffer, I want to make sure that it is 100 feet from any field in your simulation domain.

This is a nice feature to look at acute exposures. Now you are placing receptors at high density areas around all your treated fields. So you don't have a single treated field. You might have 30 or 40 fields in a given township where you would have something like this.

So you specify the buffer setback and also the spacing along the equal buffer perimeter of each receptor along that spacing. You don't even have to think about it. It generates nice curves for you.

Again, there is a trade off with this method, also. Again, you could specify really close receptor distances from each other at various buffers, but then the number of receptors increases. And I kind of at least illustrate that here.

But even if you look at the really coarse grid on the top left, that's still adequate to capture a plume leaving a field, for example.

But again, this is a trade off. I haven't on the example software that you guys have, I didn't put this in yet because I want to make it to a way that a user won't arbitrarily specify a small grid and come up with 500,000 receptors and wonder why it is not running on his machine.

So it is kind of a trade off again. When you start giving away software, you don't know who is going to be using. So you have to put some air trapping and stuff in there to make sure things like that don't happen.

Again, this will be available. It is available now. If you want, I'll give it to you now. But if not, you can wait until it formally gets loaded on the EPA website.

This gives me an example of high use density during a short time window, extreme worst-case. This is going to be some simulation results.

This is at 1,3-D township allocation. These are the fields that were selected. I put them all in and tried to get them close together. And I specified five buffer setback distances, which you can see on here.

So this program, this would be a really hard thing to generate by hand. So obviously, it is nice to have something that does this for you automatically. So now you account for 100, if you specified 100 foot buffer, you are going to get all the air concentration at 100 feet from every single field in your simulation. Again, those buffer setbacks, that's user specified.

Here is an example of some results. Obviously, the closer into the field you are, the higher air concentrations you are going to have. This could be useful to both registrants and the regulatory bodies. If they have to specify setbacks, then they can use something like this to aid them in their decisionmaking process.

Again, these are 24 hour max receptor concentrations. So each one of those receptors that were on like a 100 buffer perimeter, even though you might have simulated a year's worth of data, it would pick the highest 24-hour value.

So they might not have all occurred on the same day, but from an acute standpoint, that's what you are interested in. You want to know what was the highest value on a given day.

Here I give an example of how we use this from a chronic exposure standpoint. We use the GIS information to generate a simulation domain where we place our fields like this. You also specify uniform grid density. What I have as overlay here.

So your output results now, you know exactly the locations where your receptors are, locations where people are, population densities, mountains, et cetera. So if you're -- I'll show this example.

You could be estimating air concentrations over water bodies or sailors or urban areas or a hiker in the mountains, because now you know what those predicted air concentrations are and also the land cover type.

I show this example with about 400 receptors of simulations that Dr. van Wesenbeeck ran, typically in a three by three township domain. There is about 12,000 receptors. So if I try to draw 12,000 lines on here it would just be one dark image.

So when you have a uniform grid, you can take each receptor in that grid. You have an air concentration associated with that. Now you can plot it up in terms of like an exceedence concentration curve for chronic, subchronic, acute. Whatever you are interested in.

And now because you have data that's unique at different locations, now you can start looking at mobility and population-based risk assessment.

This is an example of how you could do that. Again, that graphic on the left, that shows the urban areas. Actually, all that magenta and different locations are urban areas. And the dots on that graph are the southwest corner of a treated field.

So it is clear on this case that most of your applications are made in rural areas. So if you really wanted to do a true population based risk assessment, you have the capability of doing it with something like this or even a mobility assessment. People can live in the city and go work in that rural region and go back to the city, et cetera.

Now I'm going to say we're taking a small step backwards. I'm going to show you how we have used this for some chronic assessment. That is where I'm going to let Dr. van Wesenbeeck take over this portion.

DR. HEERINGA: Thank you very much, Dr. Cryer. I think at this point we'll let Dr. van Wesenbeeck continue with the presentation and we'll open it up to comments on both components of this second half of the presentation.

DR. VAN WESENBEECK: I'll just have a few slides and then I'll be turning it back over to Steve.

With the California Department of Pesticide Regulations Risk Assessment, which is a probabilistic risk assessment, they make several assumptions in there. And one of the assumptions is that people live within and reside and work and spend their entire life within a single township area.

And so the way that assessment has been conducted in the past is that the probability distribution of annual average concentrations that a person may be exposed to in that township are sampled using the At Risk program, which is similar to Crystal Ball, just a different software.

And then it is assumed that a person spends a certain amount of time, for example, in their house, eight hours a day, and they are exposed to a certain concentration off that distribution. Then there is also probabilistic weight and breathing rate and other parameters that affect the risk assessment.

What we have done is we have translated that program into Crystal Ball as well, although it is not part of SOFEA right now, but the data from SOFEA can be run through this assessment.

We have also looked at alternate mobility assumptions where residents could live within and move and spend their entire lives within a three by three township domain, for example, and combinations of those.

So when they live in the central township but move around within a three township area, which we feel are still fairly conservative assessments in terms of exposure and risk, because we're assuming no one is ever leaving that three township area, let alone California or anything.

For those assessments, then we assume probability distributions. We generate probability distributions from the CDMS data that I referred to earlier. And within the SOFEA shell, you can do a custom distribution, if you have actual data.

You would select this or if you wanted to generate a theoretical probability distribution, you can generate a normal or any of these choices here.

Again, that would apply to any of the product use parameters like application rate, field size, application date, et cetera.

And in our case, we actually have data. So we use the actual data to generate our probability distributions. Then these are some examples here for Merced township. So this is a probability density function of actual field sizes in hectares in Merced.

Also application date. You can see there is very bimodal timing of application. We get very few applications in the middle of summer, but there is a peak in the fall and there is a peak in the spring time.

Again, Merced is largely sweet potato, probably 80 or 90 percent of the use in Merced is on sweet potato. So we see this really defined application timing. We may also find in different townships, in different crops, you can split these out by crop type if you wanted to.

Injection depth, this is again based on actual data. The bulk of applications were made at 18 inches. This is 46 centimeters here, 18 inches or deeper. About 10 percent of the applications are made at 12 inches. So the model shell then will select application depths according to this distribution.

And again, similarly for application rate. We see a big spike here, which is probably the most typical application rate for sweet potatoes and then there are some other crops thrown in there as well.

This is an example of the results coming up. We simulated a five by five township domain for Merced. So the central township of interest here is one which there is a very high amount of sweet potatoes grown and a fair bit of 1,3-D used.

And the application or the township allocation here is set at two, which is twice the existing township allocation of 90,250 pounds. So basically, this township is allowed to now have 180,500 pounds of 1,3-D applied.

Similarly, for these other surrounding townships here, this is based on actual historical use data over the last three to five years, I chose the highest amount of 1,3-D that was used in these surrounding townships over the past three years.

So in this case, typically we're only applying about half the township allocation. These three townships here have no 1,3-D use. And then similarly in the outer five by five townships we have the maximum use that was observed over the last three years.

A similar situation in Kern, where we had one township that had a very high requirement for 1,3-D use. So we were asking for permission to be able to -- or trying to justify increasing that use to twice the existing township allocation. And again, with actual use and surrounding townships.

So we run the model then, I believe these were

15 year simulations. So we have 1,3-D concentration here. This is annual average concentration. And we got distributions for the various scenarios. This is, the brown line is Merced with all nine townships. So, basically, it is a distribution of the concentration at each receptor in all nine of those townships. So that would be about 11,766 receptors, I believe.

And we're comparing this here to the DPR distribution that was used in their 1997 risk assessment, the black line here.

Similarly, here, if we look at just the center township only, so if we were going to run a risk assessment where we assumed that individuals' mobility was limited to just that center township, we would use these distributions here for the risk assessment.

And as I said earlier, we could use a combination of these or just one or the other.

I just wanted to talk a little bit about how long to run a simulation to ensure that we get an answer that's meaningful within certain error bounds. We haven't evaluated this really rigorously from a statistical sense.

But I think Dr. Johnson from DPR is maybe having someone look at this independently. It probably does need some more evaluation.

I ran SOFEA for a 50 year simulation for a high use township in Ventura, California. I broke that into -- each year was an independent run. So I had 50 one-year runs and then I broke that into five 10-year runs and 10 five-year runs. And I looked at the coefficient of variation at various percentiles. I also looked at a moving average of the 50th and 95th percentile, annual average 1,3-D concentrations. It is just a different way to look at it.

We haven't examined acute concentrations yet in terms of how long to run the model to get a certain confidence at a given percentile.

This is the moving averages here. So basically, run it for one year, then get an annual average. Run it another year, calculate a new annual average over those two years. Then run it a third year, calculate a new annual average over those three years, et cetera, all the way out to 50 years.

You see that as you do that at the 95th percentile, the concentration eventually starts to plateau. And it is decreasing and then reaching a plateau. At the 50th percentile, it comes down and seems to plateau earlier.

And again, I think this needs some further evaluation and explanations as to why this is different for the different percentiles.

Possibly that at the 50th percentile you get less variability in concentrations than at the high end percentiles where the concentrations are driven by proximity to treated field. So you tend to get some higher concentrations and more variation.

If I plot the coefficient variation at the 95th percentile values in this case, the yellow or greenish line here is the 50 year -- 50 one-year runs. So I take the 95th percentile concentration of each of those 50 distributions and calculate the coefficient of variation.

And it sort of spikes up at both ends of the distribution, but at the 95th percentile, it is around 10 percent.

If you do five 10-year runs or 10 five-year runs and then calculate the annual average concentration and do the CV of the 95th percentile, it comes down a little bit to five or six percent. But it looks like we're not gaining a lot by going beyond sets of 10-year runs.

So the bottom line here, again, this needs further evaluation to determine the appropriate method of determining the length of simulation. We haven't really gotten a statistician on this yet. It has just sort of been ad hoc evaluation and visual inspection of the results.

Obviously, the simulation length looks like it may need to vary depending on the percentile of interest for chronic assessment, whether it is 50th percentile or 95th percentile, and also may vary depending on whether annual average or 24-hour concentrations are needed. So that needs to be evaluated still.

I'm going to turn it back over to Steve here. There are just a few slides left, I think.

DR. CRYER: There are just a few summary slides left, it is almost lunchtime. Just bear with me. I wanted to go to -- at least where did we go from here. What we will talk about now has nothing to do with SOFEA, but I think it can address some directions on further model development. I wanted to present that here.

There are databases out there now based on aerial photography where they have been digitized by some unlucky person or persons that do exist. California is one of those location that has this type of information.

This would fully address the spatial relationship to wind direction and stuff. If you know, okay, these are the polygons for the fields. The information that we -- there are other states that have this information too.

Probably 20 years from now everybody will have it. Indiana -- there are probably three or four other states that probably have information down to this level.

But unfortunately, what we don't have is, we know where the fields are in California for this example. But we don't know did that farmer on that field make an application of this X, Y, Z product. And when did he do it, et cetera.

We have a database that tells us when an application was made. We know the general region. But we don't know the exact detail of which field that was in.

But this is the type approach if you wanted to go from a more deterministic standpoint to get to this detail -- soil databases out there too. Run a numerical model to generate a deterministic flux estimate for that soil type, et cetera, and run it, simulate it, that's how you would do that type of simulation.

It is not beyond current capability by any means. We have done similar assessments to this level for surface water, run off and stuff. But it is outside the limits of what SOFEA does and probably what most people that I'm aware of have been doing. I just wanted to point that out, that it is doable.

I don't feel you need to probably go to this detail at least at the level that we're discussing from a regulatory standpoint, but in the future that's something that could be considered.

So the benefits of SOFEA have multiple stakeholders all wanting different things. You can kind of read through that and see what they are. So you can impose different constraints and do the "what if" scenarios and see what might happen.

That's really the benefit as I see it. Not only can you simulate existing conditions, you can look at what might happen in the future. Obviously, if we all knew what would happen in the future we wouldn't be where we are today. But at least you can take educated guesses and see what that might look like.

In summary of the capability, it automates a very complex process for developing inputs, executing and summarizing necessary inputs. Again, ISCST3 -- any model that has an input output file structure can be used, basically. You just have to instead of writing a file for ISCST3, you would write a file for your model of choice. So that is just a certain subroutine in the code.

But the other capability, different crops can be simulated. What you might find is that you may not have to make broad claims about certain management practices. You may only be needing them for a specific crop like strawberries, for example.

So you can say, strawberries, these are what you are going to be imposed on from a management practice. But you don't need those management practices for another type of crop. So you have that capability now, because we keep that crop type separate.

So you can look at it and say where are my highest air concentrations coming from, they are coming from tree and vine, whatever that crop may be.

You can look at heterogeneous variable township use. That is what Ian showed on the five by five with all those different numbers in there. You might have a high use township, but neighbor to that you might not have use at all. So you can account for all that variability.

You can incorporate GIS if you have the inclination to, and we have. It is very straightforward.

Again, forecasting is possible. The system was written entirely in VBA, which is the programming language of all the Microsoft products.

So not only -- since we wrote it in VBA, then you have that capability if you wanted to generate graphics in Word or whatever or use a functionality that already exists in Excel when you are analyzing data.

There is only one module that wasn't written, that was written in FORTRAN, an Optimization code, that the VBA calls. So all of this stuff is transparent to the user. I just wanted to point that out. That was written in FORTRAN because VBA couldn't handle the programming that was necessary.

I tried it for a long time, beat my head against the wall. It turned out to be just a known error of Microsoft that they don't tell the world about, because not too many people find out about it.

But anyway, it is useful the way it stands now. There is -- I want to mention there is a FORTRAN program that has to reside in your bin directory. I think it has a solid framework for easy expansion of functionality. Again, that may be covered on our later discussion. I have ideas on that too.

So in conclusion, scientific innovation coupled with realistic assumptions allows for refinement of soil fumigant exposure, at least above a level that was never before possible.

That is the end of our presentation.

DR. HEERINGA: Thank you, Dr. Cryer and Dr. van Wesenbeeck, for your presentation.

It is 15 minutes after 12. I would like to sort of give the Panel and the audience a sense of what I would look at in timing. What I would like to do is maybe entertain about 15 minutes of questions now from panel members related to the second part of this presentation.

Then we'll break for lunch, return, and I think we have some additional time for questions, including other general questions of clarification from the morning presentation, before we turn to public comment period and then also to the actual discussion of the charge questions.

At this point, are there any questions of clarification for Dr. Cryer?

Dr. Potter and then Dr. Yates.

DR. POTTER: I have a question for Ian regarding the simulations that you did looking at the -- comparing 50 year and one year sampling stuff. How did you treat the weather in those simulations? Did you have one 50 year record that you sampled from?

DR. VAN WESENBEECK: No. We used a five-year weather record from CIMIS for Merced, sorry, for Ventura in that 50-year simulation. So it randomly chooses one of those five year weather records for each year of simulation. It wasn't a 50 year continuous record.

DR. HEERINGA: Dr. Yates.

DR. YATES: In the last couple assessment models that we looked at, they tended to be focused more on near field assessments. And there were some comments by panel members about whether reactions in the atmosphere should be included in the assessment or not.

They decided, I think the committee pretty much felt due to the short travel distances it wasn't really appropriate or necessary, maybe is a better way to say it. But in here you are now looking at much larger scales.

And I was wondering if you planned to include photodegradation or some kind of reaction with hydroxy radicals or something into the assessment, whether you have the capability and whether you have looked into that yet.

DR. CRYER: ISCST allows you to assume like a pseudo half-life degradation. If you want to lump everything together irregardless of the mechanism, you can do that. We do that or can do that.

DR. YATES: It is an input in SOFEA right now?

DR. CRYER: Yes.

DR. HEERINGA: Dr. Cohen.

DR. COHEN: When these pesticides are applied, are they always applied at the same time of the day. And I would imagine in some cases you would want to apply it later in the day so it would stay in the ground longer? I don't know.

I'm wondering if there is a variation in that, that would be another factor, I guess a variability in these emission fluxes.

DR. CRYER: SOFEA has a capability. You can specify the hour of the day an application is made, but a lot of times we don't have that detail like in the pesticide use records.

So if you can talk to ag commissioners or something that might give you a feel for what time of the day that you should choose, again, that can be a PDF also.

DR. COHEN: But are the emissions fluxes that are being used based on your measurements, are those adjusted, then, based on the time of day that has been chosen?

DR. CRYER: No, they are not. So at this stage you probably should stick with roughly the time frame that the emission fluxes were generated.

DR. COHEN: One other question, when you are varying these various application parameters, you are assuming they are all independent from one another.

I'm just wondering though if some things like application depth and application rate and crop types, it seems like they may not be completely independent. There may be some patterns that people use.

DR. CRYER: You can probably get some of that information from data mining the data. But, yes, you can specify cross-correlations with Crystal Ball if you know what they are and take the effort to find them.

DR. HEERINGA: Dr. Macdonald.

DR. MACDONALD: I have a number of issues which

I think I will wait until after lunch to raise. I was just wondering if we could get a clear printed copy of the presentation, because the most interesting slides were done with some animation and they have all printed superimposed as well as some cases where the access labels get lost in the background.

DR. CRYER: I suppose we can run to Kinkos or something.

DR. HEERINGA: Another option I might suggest, and I don't know if everybody would want that, but would it be possible just to share the presentation in Power Point and view it on your PC?

Is that sufficient or should we try to -- I think that would save everybody a lot of work. I think that's the way Peter would want it. So if you would be willing to do that, I think that might make it easier than --

Any other questions at this point from panel members. Dr. Hanna, I'm sorry.

DR. HANNA: I have a question about the multiple townships results when use zero, 1, 2, 3, 4. It looks at the highest percentiles, the number of townships, zero, one, two are very close, especially, for 24 hours. It is a little bit different for the annual, but it is still close.

But my question, even for the highest percentiles, isn't that dependent on the application process even if the zero township has less applications than township at number three or two, that might change the results that we are seeing.

Do you see it this way or am I missing something?

DR. CRYER: That particular simulation, they had the same field size and number of fields within each surrounding township randomly placed.

DR. HANNA: In reality, is this a true assumption?

DR. CRYER: Well, of course not. That's why we went to something like this were now you know the ag-capable land. Because your neighboring township might be an ocean, for example, so you can't be placing your fields there.

That was a simulation that was done three or four years ago. So now we further that to let's place fields where they need to be placed.

DR. HEERINGA: I would like to go back just to clarify for the Panel, because I know it was an important issue in the two previous meetings. That was the question about the sequencing of the flux measurements, the flux distribution.

You initiate that -- again, for chronic exposures it is less of an issue because you are rolling it over time. But for the sort of more acute exposure scenarios, it might become -- you assume that the time of day start for those off-gassing distributions is some fixed time during the day or is it just the time at which it was actually applied in the test?

DR. CRYER: It is user specified. But getting to your point, you don't want to specify it too far off from when the flux measurements are --

DR. HEERINGA: So you have a time zero and the user can say that that's 7 in the morning or 8 a.m.?

DR. CRYER: Yes.

DR. HEERINGA: Thank you for that clarification.
Any other questions?
Dr. Arya.

DR. ARYA: Following up on the same, I think in that profile of the flux, it seems to me the peak seems to be two or three days after application.

So for acute exposure, if you want to consider the maximum concentration you really have to base on the peak rather than when you applied.

DR. CRYER: The model is summarizing the 24-hour max. That might have came three days after your application or whenever. It doesn't care when it happened. That was just max concentration over the entire year simulation.

DR. ARYA: Another question, when you say acute exposure, of course, you have 24-hour average. But I would think that acute exposure will be due to hourly average. Hourly average maximums would be much higher.

DR. CRYER: Sure. That's where you get back to what you are comparing it to. You typically are comparing it to 24-hour tox value where they dose the animal for 24 hours at a constant concentration. So usually you don't have that detail.

DR. HEERINGA: Dr. Spicer.

DR. SPICER: I just had one small question. The spill-over algorithm, how often is that actually invoked?

DR. CRYER: It is only invoked if you specify I'm going to have section weighting. Then it would only be invoked if that section fills up.

DR. SPICER: So maybe in one percent of the cases or 10 or --

DR. CRYER: I don't think we have ever really seen it. Occasionally, we get a few fields that spill over.

But it gets back down to if you know -- and we do know, given the California Pesticide Use Records, where the high use areas are, they are concentrated in various areas, but then are typically not a single section. They are a few sections clustered together.

DR. HEERINGA: Dr. Bartlett.

DR. BARTLETT: This question is kind of for future development and also the possibility of applying this to the 24-hour exposure, other chemicals.

Some things I have read in your papers, one element of the probability analysis that disturbs me a little bit is the concept of what you call directional averaging as far as the probability of exposure in proximity to the field which is used for the buffer zones.

Because when -- and part of it is the communication problem, which is if somebody is 100 feet away and yet with predominant winds in a certain area with a valley or something like that, they may think, oh, I'm safe, it is 100 feet, it will never happen to me here, when we know from the modeling, we have output that there may be a predominance that from the southerly, south direction it might be 200 feet.

So with that kind of question in mind, I was noticing when you do your analysis of townships that you have something, if I'm right, is under certain scenarios you have an idea of probability or frequencies of exceedences in certain townships and certain areas.

So it seems to me it would be possible with higher resolution of grid size around a field to have probability of exceedences at different distances from the field, which would give you a better idea of what is the exposure possibilities.

In other words, we have exposure possibilities we're losing with directional averaging. You have that wealth of data that gets lost.

DR. CRYER: You are kind of mixing a couple of those papers up.

The one paper where we did the PRZM modeling, that's where we had directional averaging, that was for subchronic assessment. That was based on what some of the guidance that EPA had done in one of our risk assessments. That is how they did it based on monitoring information.

So we are trying to be consistent with what they had. Stopped using modeling information.

In terms of SOFEA, you don't get that. You get the exact concentration at a certain receptor.

What I showed in one of the last graphics, it was labeled probably acute exposure, those exceedence curves were for every receptor at the maximum concentration at every receptor at a certain setback distance regardless of where it was around the field. Do you follow?

DR. BARTLETT: Maybe you could clarify that for me, then. When you set a buffer zone of 100 feet and you say it has -- when you use this model for the chronic -- not the chronic, but the acute exposure, the 24 hour -- maybe I'm wrong. You didn't use this model for developing the 24-hour acute or not?

I notice from your history you said you changed your buffer zone in the last few years or something like that.

DR. CRYER: Right. That solely wasn't based on the use of SOFEA model changing the buffer zone. That was more based on field measurements.

But your question is if you want to get what are the air concentrations at 100 foot buffer, you have that summarized for you.

At every receptor that's 100 feet away from a field, you have that 24-hour max concentration. You have the whole gamut from the upwind versus the one that's 100 feet away from the biggest field that's surrounded by a bunch of other fields' contribution.

DR. HEERINGA: Dr. Arya and then Dr. Cohen.

DR. ARYA: Regarding this 100-foot buffer, I would like to point out that ISCST, like any Gaussian model do have limitations of how close to a source your concentration, calculated concentrations are realistic.

And generally, Gaussian models are not recommended to be applied that close to a source, you know, less than 100 meters, actually. Even the dispersion coefficients originally, Pasquel, Gifford developed, those were never given below or less than 100 meters distance from the source.

Another limitation of course was the largest distance those dispersion coefficients are applicable to. And the developers of these coefficients are based on experiments, they have always recommended they should not be used beyond 50 kilometer.

So your large domain simulation, you might be using those coefficients in excess of 200 kilometers, or 200, 300 kilometers. So you're using the capability or using the model beyond what is expected to apply.

MR. HOUTMAN: Just a comment about that. That concern was the reason that each of the field volatilities we did using the aerodynamic method, we did set up off-site air samples in each of the cardinal directions at 100 meters, sometimes 300 meters, sometimes 800 meters so we could, by measurement of air concentration, compare our ability to predict using ISCST at those near field distances.

DR. HEERINGA: Thank you, Mr. Houtman. Dr. Cohen, I believe you had a question.

DR. COHEN: Just a follow-up on Dr. Bartlett's question and also I think it was Dr. Spicer's question too about the hour question.

In some way, you are getting the directional averaging by having a 24-hour average, because it is very rare that you would have the wind blowing right toward this one receptor point for the whole 24 hours.

I mean, essentially, there could be a situation where, though, that does actually happen, and that happened two days after the chemical was applied. So you are not necessarily capturing the extreme tails of the distribution.

I mean, there could be a situation, sort of the perfect storm of exposure where somebody just gets nailed, because they just happened to be downwind when the wind happened to blow for 24 hours in that direction, et cetera, et cetera.

DR. CRYER: I misunderstood Dr. Bartlett's question. I'm sorry for that. You are right. That 24-hour max value is based on 24 hourly values that are averaged for that whole day to give you that single max value.

So you are right. You could have one hour where you had a really high peak and the next so many hours you did not.

MR. HOUTMAN: But it is a 24-hour time weighted average at a single location.

DR. HEERINGA: At this point in time, I will provide an opportunity for questions that occur to panel members as they are thinking about this or also points that the presenters or the EPA may have come to mind before we open the general question, the directed question session.

But it is just at 12:30 at this point. I would like to suggest that we break for lunch for one hour. Is one hour adequate? I'm going to poll the panel members here.

A difficulty in this location is that if you go off-site, an hour is probably not -- unless you go to the golden arches. But if you are going farther than that, an hour has not proved to be adequate.

If everyone is comfortable with an hour today, I would like to do that to stay relatively on schedule. We would return for our period of public comment. At this point in time I'm not aware that there are any scheduled public commentors.

If anybody is again interested, you have the opportunity, and speak to the Designated Federal Official, Joe Bailey, to my left here.

Let's break now and return at 1:30 or 1:35 to resume our session.

Thank you very much.

(Thereupon, a lunch break was taken.)

DR. HEERINGA: I want to welcome everyone back to the afternoon session of the first day of our two-day meeting on the Fumigant Bystander Exposure Model Review, this time focusing on the SOil Fumigant Exposure Assessment System, SOFEA, using Telone as a case study.

At this point in our agenda, we are scheduled to have public comment. I'm not aware --

DR. CRYER: Are we going to have more discussion?

DR. HEERINGA: I would like to do that after the public comment period, before the questions if we could, yes. There will be a chance for additional questions from the panel to the presenters or to the EPA before we actually launch into the formal directed questions.

At this point, we would have a period of public comments scheduled. No one has presented for public comment. So one more time we would open it up if someone is interested in a five minute public comment.

I'm not seeing any interest, I will note for the record, that we have received written comments from the

California Rural Legal Assistance Foundation and Farm Worker Justice Fund. These will be posted to the docket for this meeting, if you would like to read that.

Members of the Panel have received a photocopy. I think to clarify too for members of the panel, there was a request to have the detail of this morning's presentation broken out with some of the animation overlays.

I think due to technical issues, the way we'll handle that is to provide a CD copy of the Power Point presentation to view on your computer. That makes it most accessible.

I guess we still can't break it out in the print format. That will be supplied to the panel members who are interested on a CD format. We thank the Dow Agricultural Sciences presenters for sharing that with us.

At this point in time since we have no public comment, official public comment other than written comments that have been submitted, I would like to close the period of public comment and return before we go to the directed questions that have been posed by the EPA

Agency to the panel members, to ask the panel members if there are any additional questions that were not covered.

Any questions of fact or information, points of confusion, expansion that we would like to bring up from this morning? I'll begin with Dr. Winegar and then Dr. Majewski.

DR. WINEGAR: Yes. It is probably just a minor point. But you cite the use of the Pesticide Use Reports as part of the input. And maybe Bruce, Dr. Johnson, can have some response to this also.

I have had some personal experience in reviewing some of the reports that come in and it seemed to be a fairly high error rate from what I have seen. I'm wondering if there has been some effort to estimate how accurate that whole system is.

Do you have a feel for that? I understand not a lot of other states or maybe no other states have this type of reporting system in place. So it is probably the best we have. But I'm just wondering if you have a feel for the quality of that information. DR. JOHNSON: Yes. We have the same concern that you have on the quality of that data. I don't know if there are any studies which directly get at the question you asked, which is what percentage of errors do you have.

That's pretty labor intensive and time consuming to really work that out the way you are supposed to work it out. What we have, though, is some internal efforts to develop algorithms that check for obviously wrong values.

So there has been -- there is QC in that way. And QC in that when we find obviously wrong values, then a report goes back to the county ag commissioners that are involved, and they hopefully send us back the correct information when they get it.

So there is some lag time involved in that process. But we do look at and try to screen out the obviously wrong errors.

DR. VAN WESENBEECK: From the 1,3-D perspective, specifically, I found out that typically the uses is maxed at the maximum allowable township allocation, which is 90,250 pounds, except in cases where there have been allowances made for higher use.

So it doesn't appear that there is errors that cause it to go way out of whack. I think if something is found, then it is corrected. But I haven't found anything that seems really unusual.

DR. HEERINGA: Dr. Majewski and then Dr. Cohen.

DR. MAJEWSKI: I have a question on how you use the actual field flux data.

In California, for the shank application, you have got one field study in Salinas that you are using. Is that it? Do you have plans for other field studies to -- I don't know, get some kind of uncertainty on the per period flux values?

It just seems that you are using one set of flux data and one flux profile, you know, over the 18 day period for all your simulations regardless of where in California the simulation is targeted. Is that correct?

DR. VAN WESENBEECK: Yes. That is correct. But the flux profile is scaled based on use so that the pattern, shape of the pattern will be the same, but the actual amount of flux will increase or decrease based on the use rate relative to the rate that the study was conducted at in the field. And then it is also scaled, as Steve showed, based on depth of application and timing. At this point we're not incorporating any additional uncertainty around each of the individual flux estimates at a given time point.

DR. MAJEWSKI: Well, I guess I feel a little uncomfortable with the daily flux pattern.

You are using the same pattern across the board. And it has been my experience that fluxes vary. You do the same application in the same area, you won't get the exact same emission pattern.

And with your other field experiments, does the emission pattern, daily emission pattern look the same or am I just totally off-base and you are just looking at the total 24-hour cumulative loss.

MR. HOUTMAN: Again, the use of the model, the inputs for source strength or flux will be chemical dependent. In the case of 1,3-D, the two flux profiles selected, one to represent shank treatment, the other to represent drip irrigation, were selected because they have what appears to be a typical profile.

And again, in the case of 1,3-D specifically, to this point, the regulatory issue at hand aren't exposure durations shorter than annual average air concentration.

So what is more important for a chronic toxin is the sheer mass loss as opposed to an acute toxin where, yes, you would probably be more concerned about individual days.

But again, the profile for a shank treatment under the current conditions of use in California and other places is peak emissions occur on some day other than day one. Day two or day three is what is common.

So that profile is meant to represent really the emission profile. Then as Dr. Van Wesenbeeck mentioned it is altered based on application rate and depth and other things. It is meant to be symbolic of the emission profile, one for shank and one for drip irrigation.

DR. MAJEWSKI: Right. Yes. You say it is typical. But you get one study each, right?

MR. HOUTMAN: No. We have -- I forget the exact number, but Ian listed the number of different field volatility flux aerodynamic studies we have done. I think the total is eight.

DR. MAJEWSKI: Yes, but they are four in California and they are different allocation methods, though.

MR. HOUTMAN: Correct.

DR. MAJEWSKI: So, basically, for shank you have two field studies, am I right? DR. VAN WESENBEECK: We have three in California and one in Wisconsin. And then we have a shank bedded application in Florida. So your drip applications --

DR. MAJEWSKI: So you are using -- and according to the articles that we had to review, the major use areas are California, Washington state, Florida and North Carolina or somewhere around there?

DR. VAN WESENBEECK: Correct.

DR. MAJEWSKI: And you have a -- so are you using the Salinas flux data across the board or are you -- will there be studies -- you have one in Florida. Right?

DR. VAN WESENBEECK: Ultimately, if we can need to do some modeling in Florida, specifically, we would use the Florida flux profile. Or as you suggest, possibly trying to get at the uncertainty, we could use an average profile with a coefficient of variation around each sample point.

Those are all possibilities we could examine for the future. But we don't have a flux profile for every single region were Telone is sold in the U.S.

DR. HEERINGA: Dr. Cohen.

DR. COHEN: I wonder if you could just summarize for us, just to clarify exactly which things are being stochastically varied.

I have a list I have been generating. But I'm not sure I have everything. I guess it is thing like the weather, application rates, the field location and size, application depth.

What about the application date, like July 13th? Is that being stochastically varied too?

DR. VAN WESENBEECK: Yes.

DR. COHEN: Did I miss anything on the things that are being varied?

DR. VAN WESENBEECK: Application date, application rate, application depth, field size, type of application, shank or drip, depth of the application, whether there is tarp or no tarp on the drip application, all those can be varied according to actually measured distributions based on use data.

Yes, and field location. I didn't mention that.

DR. HEERINGA: Dr. Yates.

DR. YATES: To continue on a little bit with what Dr. Majewski said, one of the problems I see with trying to use one flux history and applying it in multiple places without having a stochastic component on it is that the flux is affected by the atmospheric stability.

And so in a sense, that flux history or that sequence is also kind of related to the history of the atmospheric stability in the atmosphere during that time period.

So then if you go and take meteorological data which has a completely different record of atmospheric stability, you are comparing maybe fluxes that occurred under, say, stable conditions in one case with now in your meteorological history you are saying is unstable atmosphere.

It seems like it would be better to try to get some kind of -- to make the flux stochastic so you don't have this kind of inconsistency between the flux and what your meteorological conditions might be from the meteorological data.

DR. HEERINGA: Dr. Spicer has a question.

DR. SPICER: Actually, I think it was answered.

DR. HEERINGA: Dr. Potter and then Dr. --

DR. POTTER: Going back to the weather and treating that stochastically, I'm wondering whether that is impacted by the length of your data record and whether you have -- you know, how you deal with that issue?

DR. VAN WESENBEECK: I'm not sure I follow the question.

DR. POTTER: If you have five years of weather and you are treating them all equally and you have one drought year in that five years, that's a one in 25 year event, if you follow the flavor of what I'm getting at, it seems like you would be oversampling or have a potential for oversampling an unusual year.

So you get into some kind of problem, I think, with having a relatively short data record for weather and sampling that stochastically, particularly when you are using a uniform distribution.

Obviously, it is a problem of trying to come up with a good data record. I don't know if you have kind of struggled with that issue a little bit.

DR. VAN WESENBEECK: We started with that as a starting point. Since it was data that DPR had used, I believe for some methylbromide modeling, there are five year CIMIS weather records, and they have been Q C'd. So we took those as a starting point. But we agree that the longer the weather record the better.

DR. HEERINGA: Dr. Potter, are you concerned in your question that by taking an entire year of actual data you have essentially represented a one year observation as opposed to sort of a more random weather pattern that might occur in the future? A prolonged drought would clearly --

DR. POTTER: Kind of both, but in terms of looking at annualized weather, typically, from a simulation perspective, we would like to have a 50-year record as opposed to a five year record to sample from in order to -- if we were going to treat that stochastically in terms of giving each year an equal probability.

So that would be my concern in terms of bias, introducing a bias into the weather record that is not reflected in the actual long term record.

DR. HEERINGA: Thank you. I guess we'll have a chance to answer this in our response questions too.

MR. HOUTMAN: Just a comment about that, each emission event is a 14-day event. By randomizing the day of application against the five years of weather, you can then simulate discrete 14-day events over the types of weather that vary within an individual year.

I don't know if that makes sense or not. But you can then look at a wider range of variability in those emissions by looking at different application dates as well.

DR. HEERINGA: Dr. Arya.

DR. ARYA: Again, clarifying the same question of stochastic representation of weather, so basically stochastic (inaudible) comes in the selection of the weather year, not in terms of weather variable. You are not selecting those stochastically.

DR. VAN WESENBEECK: That's correct. It is the weather year. But also the weather day within the year. So you really have 5 times 365 individual days when applications could take place within that year, which I think is what --

DR. ARYA: But you are still using the actual data?

DR. VAN WESENBEECK: It is actual data.

DR. ARYA: Again, the question of if you have the five years and none of those five years really experienced extreme drought or extreme wetness, so basically you will be kind of excluding those conditions and not considering longer record.

DR. VAN WESENBEECK: True. Although it is probably more wind speed and direction that are going to have a large impact on the model results rather than say rainfall or, you know, drought or whatever in this case.

DR. HEERINGA: Mr. Bartlett.

DR. BARTLETT: It seems to me that -- I'm not sure of the crops that well and the usage, but typically, there is strong seasonal patterns of planting and use.

So I'm not sure how much of it is an issue with the applications that you are dealing with. But I'm more familiar with other areas where they plant in very short periods in each region.

So I was wondering if you thought about adding some structure to the sampling of the weather and at least for quality control to see if that affects your results.

DR. VAN WESENBEECK: Well, as I showed in a couple of the slides, we do have probability distributions of the actual application timing. And we can break that out by crop type.

You are right. Sweet potatoes are typically fall, like October, applications. Other crops, tobacco might be an April application. So we have the actual dates that those applications were made based on the Pesticide Use Reporting.

And those dates then go into a probability distribution function and we sample from those. So that application timing is inherently taken into account and then matched with the appropriate weather at that time of year.

DR. HEERINGA: Dr. Macdonald.

DR. MACDONALD: I have two issues here. One, looking at this pseudo-validation, Page 47, first of all, when you presented that, I missed why it was called pseudo. I would like to hear that explained again.

Also, you are presenting there a 10-year simulation average against what appears to be one year of measurements. The 10-year simulation average is a very smooth curve which doesn't agree very well at the high end.

Would it be possible to see that graph with the simulations done a year at a time so they are more comparable with the measurements? And it would give me a better sense as to how the variability in the model is showing up in simulations.

That's the first question.

DR. CRYER: I called it a pseudo-validation for two reasons, both are addressed in your question.

The first was because we knew where the ag-capable land was, but we didn't know the exact field location in relationship to the monitoring location. So we had to make an assumption. We just put them in ag-capable land.

We might have put them in the wrong place, in the right place, we don't know. That's why we did 10 years of simulation. Because every year they got put in different places. That's why we included 10 years of data. Ideally -- also the weather, we didn't have site-specific weather for that location.

If we did, we would have used it. We didn't have that available to us. So we used neighboring weather from the next county over. And ideally, if we had that weather information, and we knew where the fields were, then it would be what I would call a validation.

It would show only that 60 days that they monitored, only that 60 days of modeling over that same time period using the exact weather that occurred during the 60 days.

We used next best thing. We approximated the inputs as best we could to mimic that scenario.

DR. MACDONALD: That makes sense, but I would also like to see how much the simulation does vary from year to year, because of the stochastic elements just to see how much spread there is and does that spread include the observed.

DR. CRYER: Sure. That's easily -- I have the data probably on my computer. You can see it tonight.

DR. MACDONALD: The other issue I wanted to raise at this point is past panels have been quite definite that Excel should not be used for random number generation.

Does Crystal Ball use its own algorithms for generating random numbers, if so, do you have any idea as to what, are there any sort of open code or are they properly documented and validated?

DR. CRYER: We use the built-in random generator in Excel. I didn't test it rigorously. We kind of saw it on my graph of 100,000 field placement locations and 50,000 over a township.

I could not see any clear clustering on that. It looked pretty random to me. So I think it's adequate for what we're doing here.

DR. MACDONALD: Yeah, well, if you are generating standard normals in Excel every 30,000 do you get a wonky value like minus 9.5? It depends on what version of Excel you have.

But we went through this in detail with panels a few years ago. And the EPA came up quite strongly saying that they shouldn't be using Excel random number generator.

DR. CRYER: If that's a recommendation, that's a trivial matter just to put a subroutine for that.

DR. HEERINGA: We'll address that, Dr. Cryer, in the response to the questions and also make sure we have citations to some of the previous reports of other panels that have covered that issue included in the minutes of the report.

Dr. Winegar.

DR. WINEGAR: I have a couple questions and comments in regards to some of the scaling and some of the flux measurements and such. It all goes under a general term, something that is dear to me as a monitoring kind of guy, and that is representativeness. Other people have raised the question about essentially the representativeness of a single flux profile for different locations around a state or different regions, et cetera.

And again, that enters into my mind from my personal experience of doing monitoring in central valley areas in California versus the coastal areas, for example. And you talk about a scaling factor for temperature between summer and winter, for example, that factor of 1.6.

And I think about what is general temperature regimes you get even within one season between a central valley and a coastal area. And that may be 1.6. You know, Kern County versus Watsonville, Monterey County or something, quite a bit different even within a season, it seems to me.

So in general, I just have a concern about the representativeness of a single flux profile.

And then secondarily, the number of scaling factors that are available to adjust for different situations.

It seems like there is a lot of personal judgment that has to go into that that essentially could allow you to tweak it and make it fit when it may not be actually based on physical parameters, you know, for example the temperature issue and such.

I was wondering if you had any comment on that.

DR. CRYER: I can at least put my two bits in. Even if you use a deterministic model, you still have to use user judgment on what are the appropriate input parameters to get it right.

What you are saying, it is a good point. Ideally, in an ideal world, we would like to have field trials all over every place we can put them. And then we have a good idea about that variability. We don't have that.

So we have to do the next best thing as scientists. We have to say what do we think is appropriate. Our assumptions could be bogus. They could be okay.

That's something we as scientists have to come to decisions on. But there are other alternatives. And the other alternative at this point is deterministic modeling.

Dr. Yates can tell us a lot more about what his group has been doing with that. But even then, like I said, there is still a lot of user judgment in picking the inputs for that, too, if you want to tweak it.

So from that standpoint it might be better to start with something that represents at least a single scenario real world conditions of what you see and then go from there.

DR. HEERINGA: Mr. Gouveia.

MR. GOUVEIA: I see here that you have randomized the location of the fields. You found a way to mine your data set for random field size and field locations. And you have randomized field locations.

Is there a way to mine the data set so that you can get an idea of how juxtaposed the fields are? My experience in the central valley and Monterey County is that a lot of these fields are juxtaposed. They are very close to each other.

Alternatively, is there a simulation that would group all the fields together in the extreme case in a single township and group them all together for a worst case scenario?

DR. CRYER: There is like two parts to that question, I think.

If you remember on my slide with the no SOFEA, you can get to that effect. You know the polygons, you know where the fields are located. But what we don't know at this time, like I mentioned, is we don't know which field got treated in that given area. It might have been more than one, et cetera.

If we had all the information then, yes, we probably could develop a system, to answer your question.

SOFEA cannot directly put everything back to back on fields. What it can do is when you specify that section weighting it will try its best to put as many as it can in a certain region.

So you are going to get pretty close to having them on top of each other. But it is not going to be exactly like butted up against each other for every field in the scenario.

DR. HEERINGA: Thank you. Dr. Maxwell.

DR. MAXWELL: I have two questions. How accurate is SOFEA from the standard plus or minus factor of two from reality of air quality models? The second question is, has any of the input data been run with other EPA models like AERMOD?

DR. VAN WESENBEECK: I think the pseudo-validation that Steve showed indicates that the annual average concentrations were getting fall within the same percentile distribution as the ARB monitoring data.

So I can't say 2X, plus or minus 2X or not, but they are within the range of what happens in reality. I think that's the best we can answer that question at this point.

And no, we haven't compared it with AERMOD at this point.

DR. HEERINGA: Dr. Cohen.

DR. COHEN: Just to follow up on that.

In the pseudo-evaluation that you did and then also in some additional data that you presented later toward the end of your presentation, in both cases it seemed like the model was underpredicting at the high end of exposure, that you were at the extreme high levels of concentration where the probability of exceedence was low.

You tended to be at the wrong side of the curve at that point. Do you have any feeling for why you are underpredicting those high levels of exposure relative to the measurements?

Essentially, they measured some high values that you are not able to get.

DR. CRYER: I wish I could answer your question. We probably could answer that better if we had the proximity of fields to the monitoring and also the actual weather. So now it could be related to a bunch of different things. And I can't say what it's related to.

DR. HEERINGA: Dr. Macdonald.

DR. MACDONALD: It is not clear to me. Is this one picture an isolated example or are we finding this consistently?

DR. CRYER: There is not a whole lot monitoring data to compare it to. This is one of the sets that we did have or that's at least publicly available.

I also did the same thing for the previous year. We have two years. I only showed this one. But they are representative. They both look more or less the same.

Again, you are going to need a whole lot of data sets to compare this to before you start making estimates on, are you overpredicting or underpredicting. All I can say is from an engineering standpoint we're well within an order of magnitude, obviously.

DR. VAN WESENBEECK: Another comment on this is that in all the modeling we have had done so far where we have had field measurements, the model has ultimately at the very highest percentile, certainly at the one hundredth percentile, predicted higher concentrations than we have ever measured.

In the tree and vine study or simulation exercise that was conducted, we had higher concentrations modeled than have ever been measured. But still within an order of magnitude.

DR. HEERINGA: Dr. Winegar.

DR. WINEGAR: This may be a suggestion in terms of the pseudo-validation, I know there is lots of methylbromide monitoring data particularly over the last few years. And in conjunction with that, a lot of usage data has been compiled.

So you wouldn't have to start from zero essentially in terms of testing out the whole scenario for different areas.

There is Monterey, there is several years of Monterey, Watsonville area. There are several years of data there, as is down in Camarillo, Oxnard. Just a suggestion possibly an area to look into.

MR. HOUTMAN: Just what we would need, though, is an understanding of the flux inputs for methylbromide, which we would suggest vary quite differently than what 1,3-D is. We would need that information as well.

DR. WINEGAR: I believe DPR has tons of that kind of data. We were talking about 30-some odd flux studies that DPR has developed over the years. It seems to me there is an abundance of flux information available also.

Is that a good assessment, Bruce?

MR. JOHNSON: There is lots of flux data. There is also a question of you would not have the CDMS data set in this case with methylbromide.

So you would have to make guesses about what application technique was being used on some particular crop. You wouldn't know that from the PUR. So there would be some guesswork involved in trying to link the actual flux profiles to the applications that you found in the PUR.

DR. WINEGAR: Just a thought, you know, a possible avenue to look into to add into validation efforts.

DR. HEERINGA: It is definitely something to think about including in a potential response to question number eight, to the directed questions.

At this point are there any additional general questions of clarification for the presenters or for the EPA?

DR. CRYER: I just have a response back to Dr. Macdonald. Bruce Johnson jogged my memory back on the random number generator.

Crystal Ball uses its own random number

generator when it samples from its PDF. So I'm not sure exactly what it uses. I used just the generic Excel one only specifically to place fields within the township.

DR. HEERINGA: Thank you very much, Dr. Cryer.

At this point, if we have no additional questions from the Panel, I'm going to turn to Jeff Dawson and ask if he has any points that he would like to raise before we enter into the directed questions.

MR. DAWSON: No, I think we're fine. Thank you.

DR. HEERINGA: If everybody is ready, I guess I would like to begin our discussion of the directed questions. And I think following the pattern of the last two sessions on the PERFUM and FEMS model, typically this will involve presentation by a lead discussant followed by associate discussants and other members of the panel.

And I think it has been instructive and productive to allow a little bit of additional exchange in the context of that with the EPA and in this case the Dow AgroSciences scientists as well.

So we'll focus on responses to the directed questions from the panel members, but we won't completely restrict it to that. If you have a rejoinder information to offer in the context of the question discussion, just please state your name into the mic and we'll hear you then.

At this point, Mr. Dawson, if you would read the first question into the record, please.

MR. DAWSON: Question one, it is focused on documentation. The background information presented to the SAP panel by SOFEA developers provides both user guidance, a technical overview of the system, and a series of case studies.

Part A, please comment on the detail and clarity of these documents.

Part B, are the descriptions of the specific model components accurate?

Part C, do the algorithms in the annotated code perform the functions as defined in this document?

Part D, please discuss any difficulties encountered with respect to loading the software and evaluating the system including the presented case studies.

DR. HEERINGA: Thank you very much. Our lead discussant on our first discussion is Dr. Scott Yates.

DR. YATES: The SOFEA model conducts exposure assessment using an Excel spreadsheet. The spreadsheet contains 17 worksheets for inputs and output. It uses a proprietary Excel based software package, Crystal Ball, to conduct the Monte Carlo analysis.

The documentation clearly states that you have to install Crystal Ball prior to operating SOFEA to make it work correctly, which is true.

There is one worksheet that is used to define most of the input and output probability density functions and other model parameters. And several are used then to include spatial and temporal information for GIS analysis of the assessment. Four of the worksheets give primary output results.

One thing that would have been nice but wasn't included, there was no real graphical output provided. Everything was columns of data, which of course could then be cut and pasted into some other contouring program or some other program to create figures.

But given that Excel has that capability, it would have been nice to have a few graphs of certain information right in the spreadsheet.

So then I have some specific things I'll go through question by question. In terms of the clarity, in general, I thought the documents were clear, provided sufficient detail to load and use SOFEA.

There are several different documents. There is users document and an install document and a programming document.

The programming manual gives a good description of -- that would be required for a user to make modifications to SOFEA.

But one of the things that leads me to think that there could be a lot of adaptations of SOFEA, and I don't know if that would cause problems in terms of which version is being used.

Although, I thought about that some. And it seems to me that ISC, if they give out the source code, people could go in and make changes and you would get all these permutations on that as well. I think when you give somebody the flexibility of changing things, you never know what is going to happen.

So in a way you could look at that as a potential weakness, but you could also look at it as a strength. So that if you decided you needed to do some modification for some reason, you have the capability of doing it.

In terms of -- in the documentation, for the user's manual, they go through spreadsheet by spreadsheet. One of the spreadsheets, the forecast spreadsheet was not -- there wasn't really any information on it, which I assume was a problem with the PDF in printing it out.

I assume that there was -- there was these large areas with no text. And I just assumed it was some kind of a printer error. But it made it hard to -- it wasn't complete from that standpoint, the documentation.

Also, there is in the spreadsheet some minor things like there are some referencing errors, addressing errors so that you get the pound sign and REF in one of the spreadsheets. But that's something easy to fix.

There are comment fields used to provide information about some of the cells in the spreadsheet. The comment fields -- I don't know if they are produced by Crystal Ball or if the authors included them.

If Crystal Ball puts them in there so that you know what the, say, the mean of the probability distribution and the range or the standard deviation, that's pretty neat. If you have to go in there and do that manually, that would be not so convenient.

There is a potential for data to be entered in more than one place. And then the data being summed, which would end up being an error, in a sense.

There are some comments that say to be careful not to do that. But that kind of -- that could be a problem for someone who is not very familiar with the system. They may end up adding two things together that shouldn't be.

In one place it should be zero and the other place it should have a value. But if you have put values in both place then they will be added together. Maybe there is some way that the program could watch for that occurring and not allow it to happen or at least bring up some kind of a warning message.

My first -- when I first started working with it, it seemed that having all the data exposed to the viewer or to the user, it seemed kind of overwhelming. And at first I kind of thought that I didn't like the idea of the Excel spreadsheet being the interface.

But I'm starting to think that maybe it is good in a sense, because when you are doing "what if" types analyses, it seems like having the data available allows you the flexibility of being able to do "what if" type forecasting.

Figure 11, the programming manual I thought was kind of confusing. They have some graphs, A, B and C that relate to some graphs that are below, but I couldn't quite make the connection in what that all meant.

Then, this is kind of a small point, the part where you talk about how fields are handled during overflow conditions. I understood it better after listening to the presentation today. But from the document it was kind of -- I kind of got the general idea, but it if it was written a little bit more clearly, I think it would have been helpful.

One thing that might be useful would be a little more description on how the program works, kind of like a flow chart that shows how execution, you know, the steps in the execution process, because there is some looping that is going on.

It is not clear to me what the steps for the evaluation process, what steps are occurring and in what order. And maybe having a little bit of description on Crystal Ball, since it is so important to the program.

The things that are essential, the way I would look at it, I guess, is that the basic manipulations that you have to go through to add the Monte Carlo flexibility into SOFEA, some of those basic things.

It might be nice to have a list of what you have to do and -- for example, one thing that I didn't really have time since I was able to get a trial version for, I think, it is like a seven day, and I didn't have time to be able to really fiddle around with it.

But I didn't see how you could take a cell that's just a standard Excel cell and create a probability distribution in that cell. And then how you make it operate -- it is probably pretty simple once you know how to do it.

So if you had some description in there, written description, I would have been able to see that without having to try to do it myself.

For part B, are specific model components accurate? I guess there is a number of things with data integrity. The fact that the spreadsheets are there for the user, there is a potential -- I have this problem when I use Excel spreadsheets that often times I go in and inadvertently change something which affects other places.

It gives me kind of this uncomfortable feeling of using Excel for things like this. Maybe it would be possible that cells that the user should not interact with should be locked so that they can't. In that way it would be more like a traditional application where you have input fields that the user can enter data and then you get output. But everything in between is kind of restricted from the user making modifications.

You could do the same thing just by locking the cells, I think, in the spreadsheet. And then if you wanted to give the flexibility to unlock cells.

In terms of describing the model components, there really wasn't any information given on ISCST3, but there were references given in the documentation to point the user to where that information is. And I think that's probably appropriate.

The same is true for Crystal Ball. Like I say, a few brief description of some of the things that Crystal Ball does might be useful, but I don't think including a significant level of written material would be the thing to do.

Some of the scaling factors that we have already talked about this a little bit, scaling the flux with depth is kind of a rough way to obtain that kind of information.

But I guess when you look at some of the simplifying assumptions in some of the other parts of the model, like using a steady state Gaussian plume model, there are some assumptions that go there. I don't know. Maybe this is in line with other components to the system.

The same is true for tarps. Tarps are strongly temperature dependent. So the temperature in the area where a fumigation occurs would strongly affect that.

And the way that you obtain the tarp -- you have come up with a 64 percent emission value for when tarps are present.

I have a little routine that calculates total emissions from shank injection with a tarp and you can vary the injection depth in the soil degradation coefficient. It is kind of an analytical solution.

But when did I this using parameters that are appropriate for 1,3-D with maybe the exception of the soil degradation, I found that if you have the injection at the surface, you get 100 percent emission without a tarp, which is sort of obvious.

But when you have a tarp present, you would get 91 percent emissions if the degradation rate was somewhat low. And if you increase the degradation rate a lot, you would get down to 76 percent. But 64 percent seemed a little bit -- I couldn't do that with any kind of reasonable numbers.

And one of the problems with -- these kind of empirical approaches too is that, I know you guys are very capable of making judgments on this, and when you see the output from a study, you recognize all the simplifying assumption that go into it. But it concerns me a little bit when this model becomes available to others, and they just kind of blindly go forth using these things and not really thinking about the consequences or the assumption that go into these.

People could create assessments that are not very meaningful and not really understand how these factors affect the output. So that's a concern of mine.

The same goes with the temporal scaling of the 1.6 factor. We already talked about that. There might be some more mechanistic ways of doing all this that might be a little bit better.

There is a detailed list of subroutines, and I think that would be really helpful for someone who wants to modify the program. I was pleased to see that. And also it would be helpful for air checking and debugging.

The third part, C, do the algorithms in the code perform the functions as defined in the document? In general, they seemed to. If you accept the assumptions for some of these simplifying things, as far as I could tell, all the functions are properly incorporated into the program.

I did get some error messages, which I'll get to in the final one, that make me wonder a little bit about problems in the algorithm. Although I'm starting to think that might be my computer.

I think that you need to include an itemized list of the modification that were made to ISCST3. I think in one of the previous panels, I forget which one now, but there were some modifications made.

They were pretty trivial, but it is kind of nice to see a list of what was changed so you can see okay they didn't really change anything of substance and then maybe have some demonstration that the form of ISCST3 that you have performs appropriately.

Then there are some potential performance issues with -- I guess Crystal Ball won't work with Windows '95 or Excel '95. And there may be just some incompatibility problems in the future when Windows changes, they always change something that you have to wonder why they did it.

Excell seems to do the same thing. Then you have SOFEA, which uses that Visual Basic and then Crystal Ball. You have a lot of different things that are trying to coexist, and when new versions come out, they may do something that causes things not to work the way they did in the previous versions.

I don't know how that's going to be handled, but it's a potential problem.

And then for the last question, any difficulties in loading the software and evaluating the system. Crystal Ball is an expensive program. But I was able to get a trial version so I could do the testing.

I found I tried to use SOFEA without installing Crystal Ball to see what would happen. And I found that some of the buttons, the ones that in the GIS part that if you want to make everything ag-capable or go back to what is in the GIS spreadsheets, some of those buttons don't work when Crystal Ball isn't installed, which is kind of surprising. But when I put it in they started working.

Since it was very clear in the documentation that you need to have this to make the system work, that's not a criticism or anything. That's just me seeing what would happen if I didn't do what I was told to do.

I did have problems running SOFEA. I was running it on a Dell laptop with a 800 megahertz microprocessor, probably 256 megabytes of memory. I kept getting this error message from Crystal Ball saying unable to complete the operation due to an unexpected error.

I would get three of them on each yearly loop. I got the error message, but it seemed like everything was fine and then would continue going on.

But then whenever there was a change in the year, you know you sampled over, I think, in the tests we had there were three years, no, two years were going to be sampled.

The first one was 1996, and then, at least in the case I had, the next one was 1999, and for some reason the file that is produced as input to ISC had some 1999 and then the data files were 1996.

And I have talked to others now since being at the meeting. Other people had it work fine. The only thing I can think of is that my computer may be not fast enough. And maybe, I don't know if this is true, this is kind of speculation, but I have worked a little bit with Visual Basic and it -- with FORTRAN it is kind of a linear process that is occurring and Visual Basic jumps around in the program depending on where execution is needed.

And I have had some programs where I tried to print output files that are then read back in. There is kind of a timing problem on a slow computer. It doesn't finish doing one thing before it jumps to do something else.

The only thing I could figure is that must have been happening on my computer, because other people pushed the run button and it just worked fine. So, I don't know. Computers, they are interesting.

But anyway, I did have some difficulty. But when I restricted the PDF so that only one year would be sampled, then it worked fine. That's partly why I think it was just my computer.

Again, I think that my thinking is changing on the use of the Excel interface, the idea of a "what if" scenario requires flexibility and this clearly gives it to the person.

And so my initial thought was that it would have been better to use some kind of a graphical interface that uses FORTRAN or Visual Basic and not a spreadsheet. But I have kind of changed my thinking on that.

And seeing the things that you showed in the presentation this morning has kind of made me think when you want to do something to see what happens and you need flexibility to change things that aren't built into some kind of a, say, Visual Basic program, you wouldn't be able to do it in this case, you can.

So I think the idea of the Excel interfaces is kind of growing on me.

And then, again, just there should probably be some kind of graphical output in the spreadsheet. That would help at least in terms of being able -- for example, if you had some graphs in the spreadsheet that we could compare to the users file, a user would know if it is working properly on their machine.

That's it.

DR. HEERINGA: Thank you very much Dr. Yates. At this point I would like to turn to the first of our associate discussants. That's Eric Winegar.

DR. WINEGAR: Dr. Yates covered everything very extensively and many of my comments he covered as well. But I do have a couple other things to add.

In general, I thought the documentation was thorough. I think I started reading at the wrong place by reading the background papers first and trying to understand everything that went into that, particularly things like the soil models and that kind of thing.

That kind of threw me for a bit. But when I got into the actual documentation of SOFEA itself, it became much more clear.

In general, the overall comment that I would make would be that the documentation is good in terms of presenting kind of a functional description of what should be done for a narrow set of situations.

It seems -- and I think it is good that you had a user manual and then a programmer's manual where you could go into some of the details of a particular function. For my relatively superficial evaluation, it seems that some advice on how to really put it all together to make a user -- is kind of like user's notes. I did see in the appendixes you had some kind of user's notes. Here is a trick to make things work better.

There is a lot of flexibility, which I think is good, a lot of opportunity to put in judgment, which you made the comment, Dr. Cryer, earlier about judgment factors. And I agree that those -- you do need to have that flexibility put in there.

I would recommend that you put in other -- your judgment advice on how to make things work well in terms of how to put the whole program together and all the different modules so that the output makes sense.

In particular, one of the things that I personally find useful is -- and Dr. Yates touched on it also, is a graphical output.

If you get a nonsensical output, even though your inputs, you look them over and think they make sense, it is really hard to glean that from a big table of numbers. But a graphical output can be much more useful there.

So either something through Excel or some way so an interface into a contour program like Surfer, whatever, a GIS sort of thing I think would be pretty useful. Particularly as it is being used by users to start developing buffer zones and the like.

Real life is more complicated than the square fields that tend to be started with, at least, in a lot of models. So the graphical output is pretty useful in viewing what is going on there.

One other comment with regards to the documentation on the clarity and kind of this user's notes kind of concept is, I notice in Section three, all the PDF parameter inputs. That's relatively -- I think in particular there, there seems to be a lot of judgment that would go into that.

So advice from those who have actually run this many times, what works and what are some good starting points and how one would go to select different options in these kind of inputs. I think that would be pretty useful.

With regards to question B, the description of the -- are the descriptions of the model components accurate? They appear to be to me.

Questions C and D, I can't comment on too much. I think it was a combination of trying to make things work on the road and a wimpy laptop. I wasn't able to really get it all together. I'll reserve comment for that later perhaps.

That's it. Thank you.

DR. HEERINGA: Thank you very much.

The next discussant is Paul Bartlett.

DR. BARTLETT: The detail and the clarity of the documents, part A, I felt that the user's manual was very clear and coincided with Excel spreadsheet. And the notes on the Excel spreadsheet were very helpful in themselves. I thought there was good documentation in the program itself.

The comment I have in general on the clarity and detail of the user and programming model is more the overview, the introduction, why are you doing this in the first place.

I also read the articles first and that's why I had a little confusion, because some of the articles were generated by field measurements and not SOFEA. And I didn't realize that.

But even if I hadn't I think it would be good to have a longer introduction of what it is capable of, what it does, what you can do with it, what are some of the common uses.

I think if you go by what you can input without knowing the model in detail and having familiarity with other models that make use of terrain and Maizo scale, you may think you are accounting for a lot of these factors in ways that you weren't.

For instance I know the model isn't using the terrain for roughness surface at this point, though it gives it the flexibility that if AERMOD or something else is used that could be incorporated, from what I understand.

So that should be clear that it has flexibility built in. As far as I know, the land covers right now just to be used for ag versus non-ag or maybe potential ag. I can see that. I like the "what if," that in certain areas you can do projections of changes in land use patterns with that information.

Again, the graphic component, if you had a graphic component with a numbered system, it makes it clear what people are doing so you don't make mistakes and put an ocean where an ocean doesn't belong or something like that, looking at numbers and that kind of thing.

Overall, I really liked -- well, it is very peculiar for me to see an Excel spreadsheet used in this manner. I'm very comfortable with using the standard data files and FORTRAN and other programs.

But I understand that we're somewhat anachronistic in this area and that it is about time that we have an interface that we don't have to spend months training graduate students in on how to do it correctly.

So a lot of my problems using the Excel sheet at first is getting used to that as a format. But I still do have what is talked about before, is quality control questions, because I'm not sure if you have written those into the routine so the program will generate errors. But I'm much more comfortable with having models that have numerous error and warning type routines in case something goes wrong.

Because when you look at an Excel spreadsheet, you are not always aware of what number might have just changed or you have a letter where you should have had a number or things like that, that errors might happen that didn't happen before.

So I guess the quality control issue.

I wasn't able to check the codes with what they were supposed to do. I presume that they do. I'm not as familiar with Visual Basic as FORTRAN.

Crystal Ball, I guess I'm not sure exactly how it fits in here, but I'll put it in the difficulties encountered section first. Because I tried to get Crystal Ball to work and I had trouble with my evaluation version.

The local authorization code didn't come through and apparently their power went out and their server went down. I was on the phone with them a lot. Eventually they got everything working.

But the concern I have with Crystal Ball more is -- and I guess with using Excel in general is transparency of knowing what it is doing what and what its limitations are. We just had an earlier discussion on seed random. I think there is a question on how the random seed is planted with one year versus multiple years and other things like that.

I would like to know a little more about that within your manuals, instead of just referring to it, like why did you use Crystal Ball. Could I get by using some other Monte Carlo type routines.

As far as the use of the model, the cost of the model is going to be a barrier. Like at universities like where I'm at, if you are not -- you have to make a pretty strong case for buying software. And if it's not going to be used a lot, it is harder to do that.

I realize there is an aversion to FORTRAN, but if there is other ways to do a module that's more open source or something like that.

But as far as the time of building such a model, I realize this is much easier to use a component like that. But I'm concerned about that being a barrier and depending on a commercial vendor for that.

So I actually didn't have any trouble with the software at all running. And that's why I was concerned they were in error. I was afraid there was an error in warning codes. But it worked fine.

But I used it to on a 2.6 gigahertz computer which is very fast with 500 megahertz RAM, which you recommended -- megahertz, I had a half a meg RAM and it ran fine. I think 14 minutes CPU time. That was pretty smooth.

DR. HEERINGA: Thank you very much, Paul.

The next discussant in the sequence is Mark Cohen.

DR. COHEN: I don't have that much to add over what has been said already. I would just like to just add a little bit regarding the quality control issue.

What struck me, for example, was one of the comments you made earlier in the presentations regarding if you were going to do the buffer analysis. But then you picked a receptor grid size that was too large, you might make the analysis invalid. And I'm just wondering -- I was looking in the user's manual for a warning to that effect.

But even better, I didn't find it. It may be there, but I didn't see it in the user's manual.

But even better would be in the program itself, that for certain key mistakes like that -- and I know we all have this problem, you can't make it completely able to not be screwed up, but you should maybe think about trying to screw it up.

Imagine that you were really making mistakes and didn't know what you were doing and go in and try to make some of the worst mistakes that you could possibly make and if you haven't already put some kind of error message in, then maybe try to do that.

In FORTRAN programs we can do that fairly easily because you can test the input.

If it's not a date, you write not a date, it should be a date. I don't know if you have that same ability within the structure of the program or not. But that would be the only thing I would add.

DR. HEERINGA: Dr. Potter, do you have --

DR. POTTER: First, I would like to commend the authors of the program for what is obviously completing a very ambitious effort, and one that I think is really neat. I was thoroughly impressed with the application using Excel as the interface for the air modeling program and found no problem installing it and running it and having fun playing with it, although I'm not sure how much time I will have to continue doing that.

So in general, the software, excellent, great job. I think it has a potential to be a really outstanding contribution to the field.

I'll turn my attention to the documentation, because I think that's the main part of the question here.

I say that I was a little bit disappointed with some of the detail in clarity in the user's manual. I thought it could benefit from some good hard-core editing and some organization that would make it a lot more readable and easy to access.

Something that I think Scott mentioned, building in some execution flow charts so we can kind of get a sense of what is going on when. And really have, you know, go into the thing with a good overview.

One of my peeves -- I spent a good time digging trying to find out what your distribution assumptions were for your agronomic parameters. Finally, having gone back into Crystal Ball, I realized that you can make custom distributions for things like depth or whatever.

But it took me a good while to find that. I spent about an hour, a hour and a half digging away, not being intimately familiar with Crystal Ball. That would be one example.

Again, if you had simply said, here are the distribution, guys, and this is what we did, we selected a custom distribution -- those are clarity issues.

You had some problems in terms of citing for your appendices. Perhaps those have been pointed out to you already or you may have picked them or up or will in your next generation.

You need to look at all your citations for your appendices, because I think there is a numbering sequence problem there that, again, I think can be quickly taken care of.

Finally, in terms clarity, I think a lot of the things that Ian said earlier in his presentation, some of the flavor of that could have been imbedded into this document. I think I would have had a somewhat easier time accessing it in terms of understanding your technical approach to setting your flux parameter.

Obviously, it is a key part of the effort. And I think building something into the user's manual explaining what your approach was and, of course, identifying what the alternatives are, which are numeric modeling or some kind of simulation effort, I think would really, I guess, kind of clear away some of the debris and make things a lot clearer.

But in general, I thought it was an excellent product. And obviously it has been produced probably under very severe deadlines. I noticed the date August on the cover of the manual. So I'm assuming you were working on it until just a short time ago. So we're looking forward to the updated versions and seeing the product as it matures further.

DR. HEERINGA: Thank you very much, Dr. Potter, and to the rest of the scheduled discussants. I want to thank Dr. Yates for leading off with such a thorough review. I think it has been a good discussion at this point.

I would also like to open it up at this point to any of the panel members or any of the prior discussants who would like to make additional comments at this point.

Not seeing any, I turn to Mr. Dawson to maybe just go systematically through to see that we have covered these questions and see if you have any further questions or need an elaboration on any of these points.

MR. DAWSON: No, I think we're fine on all four points that were raised in the question. Thank you.

DR. HEERINGA: Dr. Cryer, Dr. Van Wesenbeeck, are you fairly satisfied with the --

DR. CRYER: I think we agree. We had like a two-week not deadline, but a two-week time interval to write those up. By far, none of us even had a chance to do our own editing. I'm sure there is a lot of verbiage that shouldn't be in there.

Hopefully, you got the gist of how to use the model enough to where you could use it. We'll, obviously, refine those in the near future.

DR. HEERINGA: Thank you very much. Okay. At this point I would like to move right on to question number two, if we could.

Mr. Dawson, if you would read it into the record, please.

MR. DAWSON: Question 2, which is focused on system design and input. In the background documents, a series of detailed individual processes and components included in SOFEA are presented. The key processes include, (1) incorporation of ISCST3 into SOFEA, (2) probabilistic scaling of flux rates, (3) defining source placement within an air shed, (4) development of receptor grids within air sheds; and (5) generation of probability distribution functions based on use patterns and application parameters.

Part A of the question, please comment on these proposed processes, the nature of the components included in SOFEA and the data needed to generate an analysis using SOFEA.

Part B of the question, are there any other potential critical sources of data or methodologies that should be considered?

DR. HEERINGA: Our lead discussant for this question is Dr. Hanna.

DR. HANNA: In looking at this question, I think we have seen some of the comments relevant to the question in general. But I'll be a little bit specific about certain aspects that I feel more experienced with.

Particularly, related to the ISCST model used or inclusion in SOFEA, I think the adaptation for the case study may be a little bit need to be tuned towards ISCST capabilities in general.

By that, for example, we know that the ISCST uses the one-hour inputs for different parameters, meteorological and emission parameter or flux parameter in this case.

The most important, in my opinion, is to get the flux parameter in the current application to have an average over six-hour or so is to get it -- even impose some temporal pattern.

We have done that for some of the larger scale modeling applications for emissions, is to find a temporal pattern that can be more representative of the emission flux on an hourly basis so that can be including the ISCST.

For the scaling of the flux itself, and before I move to the scaling, also the limits for the ISCST is the distance closer to the source. That should be very much considered because, as Dr. Arya mentioned, the dispersion formula has certain limitation when you get very closer to the source. Below 100 meter we have to look and be very careful about assessment related to that.

For the scaling issue, we have seen the scaling related to the depth and related to the times of the year in terms of temperature. And that's also -- that's good in my opinion.

However, I think that also was already mentioned, the flux could be altered by the type of the conditions of the atmosphere, the stability of the atmosphere, which is not included in the process of when we choose the stochastic kind of approach of choosing the flux from a certain distribution.

The scaling itself that's a key input, not the scaling, the flux, that's a key input to the ISCST3.

The worst weather on the ISCST3 model and the emission which is the flux are the key input to the model itself.

Generally, this approach, as I see it, we are looking at how the -- in a way I look at the SOFEA application as it was presented as really is very good in addressing the uncertainty in general, the range that we can have. It is not deterministic as already was explained to us. But again, in cases of extreme conditions, you may need to be more specific about certain locations, about certain weather type and about certain situations and so on.

So I think the application is very good even with the five year, which already can be missing certain kinds of events or distribution but still very good to look at really what is the range that we can be looking at, either in the chronic or in the acute application as it was presented.

But if we are missing certain high values at the end of the spectrum by using SOFEA, that's really affected. Because that's exactly the value that we want to be concerned with.

So I would say that maybe there are certain, at least, (inaudible) or tuning related to this kind of actual condition should be added or maybe added as a kind of case studies in what has been discussed.

The receptor grids as was described, again, unless we are using less than hundred meters, it seems adequate for this presentation as we have had.

And also I like the generation of the probability distribution. That's an excellent way really to include all ranges of the uncertainty and variability, which is very important in what we are doing.

But still I think we would need more cases specific kind of application. Especially, if we are using only five years to generate this kind of stochastic input values to the SOFEA.

I think I'll stop there.

DR. HEERINGA: Thank you very much, Dr. Hanna. At this point in time I have a second discussant for this particular question, Dr. Tom Spicer.

DR. SPICER: Thank you.

The first comment I had was with regard to the scaling of the flux rates. And if I understood correctly, and I may not have understood correctly, but in addition to the parameters such as the depth and the application type and those sorts of things, they are being treated as stochastic variables, then the amount is treated as stochastic variable as well. Is that correct?

DR. VAN WESENBEECK: Yes.

DR. SPICER: That seems kind of troublesome to me, because, as was pointed out earlier, the one thing you can be certain is that for a township you are going to be applying the maximum amount that's available.

So it seems that on average, if you have a normal distribution, then on average you would expect the mass balance to close. But I think that, because the constraints associated with regulations, I think you know the total amount that's going to be applied.

So the point is that may not be the best way to take that sort of stochastic nature into account. There may be other things.

For example, it also seems to ignore the fact that if you are using, and the model does have this flexibility, if you are using either the experimental values or a soil based model, then it seems that with either of those two approaches that there are some uncertainties associated with either one of those that would be more appropriate to take into account as opposed to simply saying I'm varying the amount that I actually applied.

For example, in the experimental case, you are looking at issues of what is the uncertainties in the measurements. In the soil based model, you are looking at issues of what parameters are uncertain in the model.

So to me, there are two different things as far as flavor is concerned that simply varying the rate in a stochastic fashion, the rate at which it is applied, does not seem appropriate and seems to be almost inappropriate depending on whichever measure you are trying to use.

In fact, other parameters may be more random as has been discussed, the effect of wind speed and stability and those sorts of things.

The second thing that I had a question about or an issue about was this source placement within an airshed, this item number three.

And I don't know, I mean, when I see the word airshed, I normally think in terms of the topography how in a general terrain the wind field is going to go, how it is going to be affected by terrain and those sorts of things.

To me, the airshed idea is different than the township idea. The township idea is more just simply you are mapping off these six mile squares so that you can have some way of controlling the application of Telone.

And so to me, it is almost like you are comparing apples and oranges here in terms of defining the source placement. Because you are not necessarily considering the airshed, you are considering townships as opposed to the airsheds.

The townships may -- sorry, the airsheds may lead to issues associated with topography generated flow fields, in fact, it can get you into the issue associated with drainage flows and those sorts of things, under certain circumstances.

So those seem to be almost apples and oranges. Although, I can understand why you took the approach associated with the township.

I asked the question earlier about the spill-over algorithm. Apparently, although it is not used very often, it may very well be used in locations that are critical in the sense that it looks to me like the effect of the algorithm would be that, if you run out of a place to put a field in a given township, that what the program does is it kicks that field into the next township, in essence.

Is that correct?

DR. VAN WESENBEECK: Let me clarify that. The spill-over algorithm relates to sections, not townships. So there is 36, one-mile squared sections within a township. And typically, where we found it has kicked in, and you are right, it isn't very often at all, is in places where historically there hasn't been actually a lot of 1,3-D use.

So historically, you may only have two sections out of those 36 within the township that had any use. So each may have .5 in there. So 50 percent of the use goes into one section, 50 percent in the other.
But there may have only been 1,000 pounds of Telone applied historically. But then we're doing a "what if" scenario. What if this township goes up to the maximum allowable use.

And then we're still using that section weighting, so it is going to try and stick that maximum allowable township use into those two sections. That's when the spill-over has kicked in. That's just been due to the absence of historical data, really.

DR. SPICER: Maybe this is not an issue, then. But it seems like that what may end up happening is that you may end up actually taking use out of a section and putting it in another, in essence, distributing the same amount of material over a larger area, which would reduce the impact, reduce the predicted impact and those sorts of things.

That's all I'm suggesting, is that although it may not be invoked very often, it may be that because of when it is invoked it may be underpredicting the effect associated with the exposure in that area.

And so that's the only thing that I saw that might be difficult to associate with that.

With regard to the receptor grid development it seems you have taken two approaches. One of them is associated with the acute exposure and the other is associated with the chronic exposure.

With the acute exposure, you are basically drawing these bands 100 feet, 200 feet, et cetera. I don't see anything immediately that is an issue with that.

But with the chronic exposure, what you seem to be doing is placing the receptors in uniform grid over a larger area.

What strikes me about that is that, whereas for the acute exposure, what you are doing is you are actually putting a band about a field, and you are trying to indeed capture where the exposure might be large, by choosing an uniformly distributed receptor grid, you are first off ignoring, you're ignoring the physical locations of where the chemical starts out, that is its application points.

And furthermore, you are ignoring the position of where the population actually is. So it seems to be that, by spreading these receptors uniformly, that you have almost ignored either one or both parts of the problem that are important.

Now, I don't know exactly how to address that, but it just might be something to consider, is some way of redistributing the receptors associated with the chronic assessment.

I think the acute assessment is perfectly reasonable. But the chronic assessment just seems to be more of a problem that a uniform grid may not be appropriate for. It is just simply a question at this point.

With regard to the PDF and use patterns, this approach does indeed seem promising, although it is troublesome to learn that the PUR data is not very pure as it were.

I applaud you for trying to do that as far as that is concerned. Those are the bulk of my comments as well.

DR. HEERINGA: Thank you very much Dr. Spicer. Dr. Macdonald.

DR. MACDONALD: I don't have a lot to add here, but I do note that the critical parts of the model are based on deterministic relationships.

If these are really subject to random perturbations that aren't included in the model, the outputs will not reflect the real variability and may underestimate the higher quantiles of exposure.

I'm not an expert in these processes, but I certainly would like some assurance when deterministic relationships go in that the variability about those relationships is not important.

DR. HEERINGA: Thank you very much. I think some of these issues are going to come back again in question three when we discuss the flux.

So I'm sure we'll have plenty of attention paid to that.

Are there any additional comments from other members of the panel who are not scheduled as a primary or an associate discussant? Dr. Arya.

DR. ARYA: I would like to basically propose that, while this term was mentioned airshed, probably derived from watershed, where watershed is more clearly defined, the area in which the water from, you know, where the water is kind of confined to a tributary or river. But air does not follow those kinds of rules.

So probably instead of airshed we should use the model domain. How did you define the model domain? It should be based on probably the capability of the model ISCST we are using.

And again, as mentioned, one of the limitations is that you should not, one should not use the model more than 50 kilometer away from the source or maybe in some cases they are extended to 100. But it is never recommended beyond that, you know.

So I would say that you sort of confine your model domain keeping that in mind, so long as you are using this ISCST or even AERMOD, which are kind of based on almost same short-range type of dispersion ideas.

I think I had another question about the receptor, placement of receptor, for example. When you have, of course, receptors on a uniform grids, some of the receptors will lie, of course, in the treated fields.

And they don't --

DR. VAN WESENBEECK: They are excluded.

DR. ARYA: Somewhere I read they are excluded only for seven days, but after seven days they are, even though there may be emission after seven days.

Because according to your emission model, the emissions, you know, last up to 14 days, at least.

DR. VAN WESENBEECK: Right. But the reentry on the label is seven days so people could be walking over that field. So it is representative of people moving about the township. That's one location they could potentially be.

DR. ARYA: Thanks for clarifying that.

DR. HEERINGA: Just to be clear after a seven day period in the model, the receptors on fields are reactivated and incorporated.

DR. VAN WESENBEECK: Yes. And the user can specify that time period depending on the specific molecule on the label.

DR. HEERINGA: Thank you. Dr. Potter.

DR. POTTER: Yes, I just had one thought about the receptor placement. That came after I looked at work that Jim Seiber had published a few years ago. I think I know a few guys cited it.

But when they looked at some of their monitoring data for methylbromide, they used the same model too, they used flux terms and dispersed the chemical using this ISCST, or whatever the acronym is called, model.

They had some good success and bad. Sort of fell in-between. Sometimes it was underpredicting, sometimes it was overpredicting. A lot depended on the flux. So obviously, we'll get to that in the next question.

But one of the things that at least would turn the light on in terms of their data is that they get particularly higher concentrations in the monitoring data at higher elevation when they had fields that were pushed up against the mountains or something like that in the airshed that they were working in.

In terms of receptor placement, I think obviously topography, maybe not in the scenario we're talking about, is important. But certainly it will be in other areas where topography will play a very important role in terms of potential for exposure.

DR. HEERINGA: Any other questions at this point or comments?

Just go back, I guess if we could, before we move on review the elements of this question. One of them was the incorporation of ISCST3 into SOFEA. And I think that -- I haven't heard any serious concerns about that.

There was a mention earlier about documenting the small modifications in the actual programming code that had been made. Probabilistic scaling of flux rates, any additional comments on that aspect of this question?

Source placement, we have had a fair amount of discussion on this. But that obviously becomes a little more complicated with the multi-source aerial perspective of this model.

Receptor grids with an airshed is kind of a continuation of that same issue in terms of not just single source but multi-source. Any additional comments? And then, finally, the generation of the probability distribution functions on use patterns and application rates.

MR. HOUTMAN: I just wanted to make a comment about the receptor grid in the spacing of those receptors.

The attempt is to determine a mathematical average of the air concentrations across the townships or understand the distribution of air concentrations across sections and then permit people under different exposure scenarios and mobility assumptions and then move amongst those receptors.

But the equal spacing and the designation of them uniformly across an area is just for the determination of air concentration distribution. And then the exposure component then is laid on top of that with mobility assumptions and other things.

The other thing about airshed versus townships is townships were selected for their administrative convenience in their uniform sizes. We mentioned earlier there are township allocation limits, amount used per year.

Well, counties are irregularly shaped and airsheds are irregularly shaped. Townships were something that is of standard sizing, of a size that permitted it to be a good candidate in order to regulate product use densities. That's why townships were selected.

DR. HEERINGA: Thank you for that clarification. Just a follow-up with regard to your receptor sites.

When it comes to risk assessment, you actually move people to the receptor. You don't have to have the receptor find the person. You sort of essentially lay out a life style, a mobility pattern, within that area that associates individuals or populations with receptors then?

MR. HOUTMAN: Correct. The receptors are only to define air concentrations and then population mobility and location is then interfaced with that.

DR. HEERINGA: Dr. Cohen.

DR. COHEN: There is just one comment about the insertion of the ISCST3 model in this type of application.

If you have a situation where the wind is blowing fairly slow and you do your hourly calculation, it sort of assumes that the material is dispersed over the entire gaussian plume domain.

And then the next hour -- what would happen in reality if the wind changed direction and that same material started getting blown back and maybe hit a receptor again?

You would miss that, I think, in your using the model the way you are using it. Because the next hour, the wind direction would just be somewhere else and the material's just going to be off in that plume.

But actually what happened -- it could be that somebody could get hit on the way out and then on the way back if the wind were to change direction like that.

I'm not sure if that's very clear.

DR. CRYER: We tried to address that a little bit earlier. I forgot who it was, maybe Mr. Bartlett, but you are right. That can happen. You can track that in

ISCST3, and you are going to lose it when you get a 24-hour value.

But again, the details of the tox information that we're ultimately using to compare this to we don't have one-hour exposures. Typically they dose a rat for 24 hours.

So you really have to come up with it would make sense to use a 24-hour average value for your exposure prediction in that case. When we get that information in the point where they are dosing rats for an hour, then, yes, we have to go to that detail.

DR. COHEN: But this approach isn't really even getting the correct 24-hour average, though, in the situation of the wind diversion direction. Because the plume is just moved to a whole other vector each hour.

One hour it is going in this direction and you have concentrations out and the receptor's downwind of the plume. The next hour, if the wind changes by 30 degrees or something, you are getting a whole other vector of concentrations.

See, if the next hour the wind changes direction

180 degrees, the model is just going to assume that you are going to get concentrations out going the other way on the other side of where the first hour was.

But in reality what is going to happen is that high concentration that existed maybe in that first hour is just going to come right back and the people are going to breath it again.

You are missing that. It is not just that you are just averaging it. It is more than that. You are actually moving the plume back to the center line -- to the emissions point and shooting it off the other direction.

DR. CRYER: Obviously, all those scenarios can potentially be feasible. I don't know how often that would occur.

If you have a plume that moves out and it is over the top of that receptor for that hour, it is going to log that concentration.

Say it never moves again, never disperses, then for the next 23 hours it is going to record that same concentration. So for 24 hours you are going to have a higher concentration than if it moved.

Do you see what I'm saying?

Again, it gets back to, sure, a system like this can give you those details if you want to get down to the hour basis, but, really, what are you going to do with that information when you have it. That's really where I'm coming from.

DR. HEERINGA: Dr. Gouveia and Dr. Arya.

DR. ARYA: I just wanted to comment on the same. I think that's an excellent comment, basically, on the limitation of any of the analytical dispersion models, Gaussian model, ISC included.

And these models treat basically one hour of the emission and transport and dispersion. But they do not follow really how it is connected to the next hour, you know the material that went in the previous hour. It is not being followed in the next hour.

In the numerical models, like these reasonable models, even short-range, numerical models, the air quality included in there, they can do that. Because they are kind of continuous in time. And they treat all the receptor points really as a function of time.

So they will have in them the material following in and out that occurs.

But ISC will not be, I think, by its very nature is not able to handle that.

DR. HEERINGA: Dr. Gouveia.

DR. GOUVEIA: I would support what Dr. Arya said. The next stage, next level of modeling that would handle what Dr. Cohen is suggesting is a mass consistent model, MATHU (ph), particle and cell modeling that's done at our facility at ARAK (ph). But it is not for regulatory use and not for -- it takes several meteorologists to run correctly.

But ISC is still the standard workhorse for this type of modeling. The limitations -- there are limitations because it is straight line. It is analytical. But it is something that everybody uses because it has a wide range of uses.

DR. HEERINGA: Given those comments, but with general consensus with regard to the ISCST3 model is that its use is appropriate in this recognizing the limitations or do we think there are -- DR. GOUVEIA: We have to be aware of all the limitations, and there is many of ISC. We'll probably talk about more of them in -- I think it is question four coming up. And Dr. Arya pointed out a few.

The 100 meter limitation on very near field dispersion is -- the science is just not there to correctly handle it. There is another feature.

DR. HEERINGA: Thank you very much.

DR. BARTLETT: Just one comment on that. It does seem like what is brought up is an underestimate of concentration, this phenomenon. We haven't talked on these panels before about this broad use of the Gaussian plume model with multiple sources in a larger area.

So we really do need to know how significant that underestimation might be for these instances of wind shifts and one hour time periods.

DR. HEERINGA: Dr. Spicer.

DR. SPICER: I agree with that. In addition to the issue associated with modeling, you also have exactly the issue associated with wind field and having a consistent wind field picture, which is exactly what MATHU (ph) and ADPICK (ph) can do as far as that is concerned.

The other question that I was suggesting associated with the regular receptor placement, and I think this goes back to the general application of the modeling.

It is kind of an independent question than the test case itself that we're talking about. I think that -- as I said earlier, the idea of using the regular receptor grid around the field and using ISCST3 does give you a reasonable estimate of the acute exposure.

And I was simply questioning whether the regular placement of the receptors was going to give you the long-term exposure. I mean, another way you could think about it is you could choose your receptor grid based on the predicted value. Suppose that you chose a predicted value based on the maximum value at your exclusion distance, so that you chose a receptor, instead of at the regular grid location, you chose, at your exclusion zone distance, you chose that position as your receptor.

Then I would be willing to bet that the model predictions would generate much higher concentrations. Because you would be choosing receptors which you would be basically interpolating your population movement associated with. They would have higher concentrations than just a grid that was distributed regularly across the terrain. That's one of the reasons why I think that this regular distribution of receptors for the long-term exposure may not be conservative.

DR. HEERINGA: Dr. Cohen.

DR. COHEN: A final comment from --at least, from me on this point. There was a study I found where they compared the ISC model runs with runs of CALPUFF, which is sort of a puff dispersion model similar to the one that Dr. Gouveia mentioned.

And they did find, in fact, that the ISC model did underpredict the concentrations because of these types of situations. There were conditions that they ran where you got much higher maximum with the CALPUFF model because of this treatment of -- the pollutant isn't just lost after each hour. That it can stay in the same place and keep building up.

So there is some quantitative information in this report about the levels of underprediction or overprediction. But you probably have to do a bit more to quantify this more. But I think it is real.

DR. HEERINGA: At this point I would like to turn to Mr. Dawson, see whether he feels we have covered this question and its components, whether there is anything at this point you would like to ask about.

MR. DAWSON: I actually had a number of clarifications, if you could bear with me.

I guess I'll start with the simplest first. Following up from Dr. Arya's comment about the workable, I guess what I classify as workable ranges of ISC.

So I guess the minimum working range it seems like is 100 meters or so and that's based on the nature of the dispersion coefficients used in the calculation. Is that correct?

DR. ARYA: Well, that, also the fact that in general that usually these Gaussian-based models are not applicable very close to the source because they don't include any upstream dispersion.

According to Gaussian model, your concentration is zero just a short distance or even slight distance upwind. In reality because of diffusion and turbulence, there is always upstream air concentrations too.

Material disperses upstream also to some extent.

MR. DAWSON: And then the maximum workable range you were saying was 15 or 50?

DR. ARYA: Fifty. That comes from the ISCST, they use the Pasquel Gifford dispersion curves. And those were developed from experimental data, which really the range was limited to 50 kilometers.

MR. DAWSON: As far as the -- there is a lot of discussion here about one hour versus 24 hours. We're concerned about the basic methodology. Because, for example, for most of the -- the previous cases we presented, it was 24-hour issues that we were talking about.

But we do have some of these other cases where the threshold value is really based on one hour of exposure. So we're also potentially interested in durations as low as one hour. We understand that's the lowest you can go with ISCST.

As we go further into the discussion, if we could carry that concept along, as well as the chronic, up to the chronic levels of durations of exposure, that would be great for you guys to consider.

DR. HEERINGA: I was going to reinforce that point too. Question seven specifically addresses that issue of length of exposure. The case study here, of course, relates to chronic exposure on a one year average.

I think, clearly, as we get into this discussion the focus will be on the generalizability of SOFEA to the acute situations and shorter term exposure. I think all of the panel members have that in mind.

We'll definitely collect that response in response to number seven too.

MR. DAWSON: And even down to one hour. Because we didn't -- I think in the previous sessions we were talking about 24 hours. I guess the one hour didn't come up as much.

On the third one, I would like to follow up with

Dr. Spicer's comment. I think it was his first comment about the stochastic scaling for application rate versus total mass, if I'm saying that correctly.

And in this particular case for this chemical, there is a township cap, but in cases where we would potentially generically apply this methodology where there is no township cap, so you don't have a potential limitation on the total mass applied, how would that impact your comment or your thoughts about that process?

DR. SPICER: What I was trying to say was that including the stochastic nature in the rate at which you apply the material seems to me that you are applying that variation in exactly the wrong place.

And based on what Dr. Macdonald said earlier, if you have a deterministic model and you don't include the uncertainty properly, then you can improperly predict details of the distribution. So his point is well taken in that regard.

All I'm suggesting is that one of the few things that the farmer may very well know is how much he distributed during the course of the day.

So that just seems to me to be the wrong way to include that variability, that there are other more appropriate ways to do it even if you do -- whether you have a cap or not.

MR. DAWSON: We were talking here on the receptor grid issue. It is still somewhat unclear to us the implications of -- that there is a clear message especially on the placement of the receptor grids and the longer duration exposure scenario.

So I don't know how to ask for it except for is there any more kind of clarification that could be added with regards to that issue?

DR. HEERINGA: Specifically, are you asking about why -- I think there was a statement here to the effect that the uniform allocation of receptor grids over the sort of the estimation space might lead to an underestimation of the distributional concentrations. Is there somebody who would like to address that?

DR. SPICER: I'll try again.

All I was trying to suggest was that if you looked at the acute exposure, what you are doing is you're drawing a band about your field that may be, for the sake of example, 100 meters away.

Let's say that's what your exclusion zone is after you apply it. You have a receptor location at that point. You predict the exposure at that receptor. All I'm suggesting is that the concentration at that receptor would tend to be higher than a receptor that was on a uniform spatial grid simply because it is located near where the material was put out.

And so if you have a series of those receptors that are located near the fields where you have got applications, then you would end up having, it looks to me like -- because you are choosing the receptors in those locations, you would end up with a larger concentration.

Now if you have a uniform grid and you have random fields placed throughout, then some of the receptors are obviously going to be excluded because of the exclusion zones.

Other receptors are not necessarily going to be located near fields. And so there is a way that you could actually choose the location of the receptors which would increase the amount of exposure that's being predicted.

MR. DAWSON: So in essence, if I'm interpreting this correctly, you are suggesting a weighted receptor grid to the source approach?

DR. SPICER: Exactly.

MR. DAWSON: I guess, I'm sorry -- the last clarification was at the end there was a lot of discussion about what seems to be the overall applicability or implications of using ISCST versus other potential modeling approaches, including ones that address more or less a mass balance type of approach.

I guess a couple clarifications in there. One, is it appropriate. I mean, is this the most appropriate thing to do compared with the other potential models that are out there.

And I guess we'll get to this as well in some of the later discussion, but what are the implications or what are the inherent biases in there as far as overestimation or underestimation of exposure because of the use of ISC.

DR. HEERINGA: Would a member of the panel would like to address this issue, the appropriateness of ISC relative to alternatives? I guess feasible alternatives for the purpose of this point. Dr. Spicer?

DR. SPICER: I think that as far as acute hazards are concerned, that ISCST3 is probably a reasonable choice for this because of its track record and those sorts of things. And it has not been challenged. The difference that we have in this situation is this idea of chronic exposure.

I think what has been pointed out earlier in terms of using either other sorts of models like CALPUFF, for example, that model might indeed have an advantage over a longer term.

Now, of course the problem that you get into is that you can take -- what you have now as far as weather data is concerned, you have this stochastic weather data at one particular location.

I mean, that's an issue as to how well the weather data would fit in terms of applying it to many locations.

So it is uncertain.

DR. HEERINGA: Dr. Cohen.

DR. COHEN: Just to add to Dr. Spicer. I'm not sure I would agree with you, Dr. Spicer, in terms of that it is okay for the acute but -- excuse me, okay for the acute but not okay for the chronic. I think some of the same problems we have been talking about occurred for the acute as well.

The problem I raised earlier that we talked about, the problem of the wind shifting direction and the calms and things like that affect your acute concentrations as well. So I think it affects both.

DR. SPICER: Certainly. For the record I agree with you completely. In fact, the calms and things like the effects of topography that I was talking about earlier may be the most important things and neither of those were taken into account with ISC.

MR. DAWSON: Just one final clarification on the very near field, less than 100 meters type of scenarios. What are the potential implications there in applying ISC for lack of a better tool, for example?

DR. HEERINGA: Dr. Arya.

DR. ARYA: I think you will consider that only if there is a limit, there is existing buffer zone less than 100 meter. If that is allowed then, you may probably want to determine concentrations less than that.

But I think the closer to the source you are, the higher the concentration you are going to get. But whether this model ISC or any Gaussian model is really capable of doing that, that's questionable.

MR. DAWSON: As a follow-up to that, do you believe, let's say, for example, at 50 meters you may over or underpredict exposure with the ISC?

DR. ARYA: Well, you know, one thing is that maybe the dispersion coefficient that are used in ISC may not be applicable at short distances. At short distances actually rate of dispersion is faster than usually that's used in ISC.

So on that point of view, ISC might be giving more conservative estimate actually.

DR. HEERINGA: Dr. Winegar.

DR. WINEGAR: There was a paper in the Air and Waste Management Journal in April of 2004 by ARB people and UC Riverside, I believe. Yes, where they did a near --- called Near Field Dispersion Modeling for Regulatory Applications, and they released a tracer from a trailer and then did sampling less than 100 yards downwind.

And they got pretty good agreement actually in that short term as long as they included some factors related to the meandering of the wind direction in that short distance between the source and the receptors.

So anyway, I can include this reference in my comments so that you can look at that and that might be useful to think about.

MR. DAWSON: That would be great. Thank you.

DR. HEERINGA: Since you have a copy of your journal, I wonder, would you be willing to loan it to the Megatech folks? We'll have copies made for people. There is no use to wait for the reference. That's great. I'm sure people have it. But we may as well make copies for people to look at that.

DR. HOUTMAN: Did they use ISCST to predict those near field --

DR. WINEGAR: They did some modification. I don't remember exactly what, how they did it. I think they actually modified some of the dispersion coefficients.

Frankly, I just kind of scanned through it. And I don't remember all the details, but they basically -- ISC was the basis for it and they did some small tweaking, but I can't really say what.

DR. HEERINGA: We'll try to have copies of that available by tomorrow morning so people can look at it.

Any other, Mr. Dawson, any additional clarifications?

MR. DAWSON: No, I think that will do us. Thank you.

DR. HEERINGA: I'm sure we can revisit some of these. We'll have a general session at the end for anything that we have managed to miss on our first pass through the questions.

At this point in time, I would like in terms of our agenda to move on this afternoon yet to address question three. But I think all of us are due a 15 minute break. So I would like to ask maybe if we could all convene back here at five minutes of 4?

(Thereupon, a break was taken.)

DR. HEERINGA: Let's resume for the final part of our first afternoon session on the SOFEA model using Telone as a case study. We are in the question, direct question period, and we are up to question Number 3. If I could ask Mr. Dawson to read question Number 3 into the record, please.

MR. DAWSON: Question 3. The determination of appropriate flux and emission rates is critical to the proper use of the SOFEA model as these values define the source of fumigants in the air that can lead to exposures.

Upon its review of how flux rates can be calculated, the Agency has identified a number of questions it would like the panel to consider.

In SOFEA, measured flux rates specific to the conditions at the time of the monitoring studies used are adjusted based on incorporation depth and seasonal differences to account for varying application conditions.

Emissions of 1,3-D are sensitive to soil temperature and incorporation depth. Incorporation depth is addressed using the EPA model PRZM3 and also the USDA model CHAIN-2D. Scaling factors were used to address temperature differences.

Part A, what, if any, refinements are needed for this process including the manner in which flux values were directly monitored and calculated using the aerodynamic flux approach?

Part B, SOFEA can easily be modified to probabilistically vary flux rate for each application based on variability in field flux measurements. For example, application method or temperature, or model generated flux. Please comment on these potential modifications.

Part C, How appropriate is it to use a flux or emission factor from a single monitoring study, or small number of studies, and apply it to different situations such as for the same crop in a different region of the country?

Part D, Please comment on SOFEA's capability to adequately consider multiple, linked application events on an airshed basis as well as single source scenarios.

And finally, subpart E, does SOFEA appropriately address situations where data are missing?

DR. HEERINGA: Thank you very much, Mr. Dawson. Dr. Majewski is the lead discussant on this particular question.

DR. MAJEWSKI: What, if any, refinements are needed in the aerodynamic flux approach?

Well, as I mentioned earlier, I haven't kept up with the literature on what is being done to fine tune the atmospheric stability descriptions. I was wondering if you had, because all the papers I'm familiar with are at least 10 years old.

That would be one suggestion, is have any improvements in atmospheric stability correction terms been published? Has there been any work on that?

And then the second is the sampling, the actual sampling period. As I mentioned earlier, I think the 6/6/ and 12 approach is pretty coarse.

For example, if you sample from 6 a.m. to noon and from noon to 6 p.m. and 6 p.m. to 6 a.m., you are including unstable conditions with stable conditions and you're kind of attenuating the stability influence on the actual fluxes, plus you only have three data points per day.

I think you are missing the variability in the fluxes that are occurring that typically are increased from sun up to noon and then decrease afterwards, depending on soil moisture, of course, and temperature in that area of the country.

I like the fact that you are using measured flux values. And the fact that you have incorporated PRZM or the CHAIN2-D models as an additional potential source of a flux term, is I think the way to go.

Although I'm not that familiar with these two models, I'm sure the panel members can comment more on that. I think the CHAIN2-D is probably more appropriate than the PRZMs.

Moving on to B, SOFEA can be easily modified to probabilistically vary flux rates. Comment on this potential modification.

It seems that the only real adjustments to the flux term is based on application depth and temperature.

And I guess application rate as well. And that's probably the primary components that affect the actual flux.

But I still feel a little uncomfortable just using the scaling factors based on one or two field studies in California for the California studies.

For the other studies I'll address those in part C. But the fluxes have to be adjusted according to different times of the year, application rates and whatnot. And I just feel a little uncomfortable on that the user selected scaling factors.

And then moving on to C. How appropriate is it to use flux emission factors from a single monitoring study and apply it to different situations for the same crop in different regions of the country.

I don't think this is appropriate at all. There is a lot of environmental factors that affect volatilization fluxes. And you are using data from one or two studies that are conducted in one or two areas of a country and trying to apply these emission values in other regions of the country where the soil situation is different, soil moisture, organic carbon, rainfall, humidity, temperature, air temperatures, things like that.

All these factors go into what affects the volatilization flux. And all these factors are taken into account when you measure, when you do your field experiment. And then you take these results and put it someplace else where the environment is significantly different.

I think there is an inherent error someplace in there that is not being accounted for in transposing the basic flux values to other parts of the country.

Then SOFEA's capability to adequately consider multiple linked application events on an airshed basis as well as single source scenarios.

I think it does a good job at least from my limited modeling experience, based on what I have read, the documents you provided for us for this panel. And in places like California where the pesticide use data is pretty extensive, I think it works.

But how applicable is this to other parts of the country where -- like Florida or Washington where they don't have as extensive a pesticide use database. I think you may run into problems with that.

And then does SOFEA appropriately address situations where data are missing. I couldn't find much on this in the documentation. So I'll have to defer to my other panel members here on that issue.

DR. HEERINGA: Thank you very much. At this point associate discussant, the first is Dr. Ou.

DR. OU: My comment will be very general, not cover all the question. And to begin with, I would like to point out that the many study of chemical have different chemical, physical property, biological property and toxicological property.

And cis 1,3-D and trans 1,3-D are no exception.

Personally, I consider cis and trans 1,3-D are two different chemicals. You may not agree with me. Anyway, I have on and off interest in this chemical for more than 20 years. In Florida's sandy soil, the degradation rate in live soil and sterile soil seemed to be about the same. As a microbiologist, I was very puzzled why they are the same.

Since one's with the microorganism and the other have dead microorganism, and degradation are the same. And I realized, also McCall's publication, chemical hydrolysis in water. I realized the (inaudible) biodegradation in soils, chemical hydrolysis. Can I have first slide?

Where the cis and the trans have been degraded, you know, separated into corresponding cis and trans-3-chloroallyl alcohol. And the non-enhanced soil is transported away (ph) by the chemical. In the data, I found out if I am correct, a soil from a site just have been repeat treated with 1,3-D and the one that -- also (inaudible) soil actually faster than sterile soil. Of course that we know idea of enhanced degradation.

Also trans 1,3-D has been degraded faster than cis 1,3-D. Also hydrolysis through -- cis and trans chloroallyl alcohol have not been degraded, just been the biological correspondent of cis and trans-3-chloroallyl alcohol in addenda, become organic S in (inaudible) and water.

My point is, of course, have been published, enhanced degradation of 1,3 for more than 10 years. First provide the scientist and then (inaudible).

I'm curious about this, I do not get any (inaudible) about chloroallyl alcohol whatsoever except on biodegradation since I publish (inaudible) about biodegradation on chloroallyl alcohol.

But I could not find anything about physical chemical property and toxicology properties.

Can I have the second slide, also shown by Dr. Wesenbeeck, but area 40 degrees and a half-life, just a chemical hydrolysis 40 degrees, half life only .8 days.

And I point out this because under Florida condition you have the field has been covered with the plastic and the plastic temperature can get to 45 degrees. And the sandy soil can be around 40 degrees. So the chemical hydrolysis under this situation may be measured (ph) due to degradation and, of course, can go to the chloroallyl alcohol.

Since nobody know about the physicochemical property, how volatile the chloroallyl alcohol, nobody knows, except I wonder the similar chemical, it is called

2 chloropropene-1-ol, which is similar to the alcohol. It have a boiling point 133 degrees. So it could be somewhat volatile, not as volatile as 1,3-D.

In a sense, we don't know anything about -- I don't know about chloroallyl alcohol. Since I don't have an idea about chloroallyl alcohol, I don't know about volatility on 3-chloroallyl alcohol. So I don't know contribution of toxicity exposure. I don't know.

Anyway, the other thing I want to point out -- can I have the third slide.

The enhanced that showed in the Florida sandy soil about cis, how cis and the trans is about the same. But enhanced soil, trans degraded faster than cis.

But in a situation, chance being that trans 1,3-D been degrade faster. And also Dr. Wesenbeeck mentioned that cis 1,3-D is more volatile that trans.

We find that after injection, 1,3 soil. Cis 1,3 always faster come out once volatilized. Usually, one to five hours after injection and it ended one, two, three hours after that. Trans 1,3-D had been volatilized.

And in the enhanced soil trans degrade faster.

So the vapor in the air would remain at cis -- we find in the non enhanced soil, biodegradation about in the first twenty hours, about 1.5 3 ratio. So we are initially the most there would is a 1:3 into atmosphere. But in enhanced soil it would be much more. We have not determined the enhanced soil, so I have no idea. But I consider it would be -- the ratio would be higher in the enhanced soil.

The other point -- can I have the fourth slide? I don't have any idea about -- since I'm not a toxicologist, I don't have any idea about the toxicity, human equivalent toxicity for the cis 1,3-D and trans 1,3-D, I don't know. Except that I know the cis 1,3-D is much more toxic to a nematode.

In fact, the scientist favor the use of cis 1,3-D alone for the control of the nematode. Because they consider the trans 1,3-D useless.

So the question is if the cis 1,3-D is more toxic than trans 1,3-D, you have to take into account the toxicological factors. If cis 1,3-D is more toxic than trans 1,3-D, then you have to take into account the toxicity difference rather than just take around -- cis 1,3-D and trans 1,3-D together as one, because we are considering exposure.

And if two chemicals have a different toxicity, you have to consider the different toxicity rather than consider it as just one chemical.

And of course you enhance degradation effects the flux. You have more flux for the cis 1,3-D than trans 1,3-D. That's my point.

And the toxicity is unknown. So for me -- I know some people maybe know the toxicity. So that's my comment.

DR. VAN WESENBEECK: Just at least in partial response to some of those comments.

You are you correct cis 1,3-D does come off a bit sooner. We see that in the field studies too. But we do of course collect all the vapor in one tube but then we analyze separately for cis and trans.

But we never see a factor of three or anything like that difference. It's usually just a few percent, and the other point is I think tox studies are conducted on the mixture.

So that would inherently be taken into account in terms of the tox endpoints.

DR. OU: The only soil we have dealt with this is in Florida sandy soil, we found that after four days. We also take soil sample up to 19 centimeter depth and determine a 1:3 ratio. After four days, we expect not much 1,3-D in the soil. We don't expect it to continue to volatilize.

So in the Florida -- in the sandy soil, we're up to four days with that assumption covered with the (inaudible) permeable or cover with the P or no cover after four days not much were (ph) come out from the soil.

DR. HEERINGA: I think we can -- certainly, the slides that Dr. Ou has presented, that material will be incorporated in the response.

And I guess if we'll have an opportunity if you want to think about it some more, talk to him to respond tomorrow. But as I gather, the point here is that as soil types differ, the cis/trans isomers have different behaviors, but your point is that you are measuring toxicity and haven't really observed differential, extremely differential off-gassing rates for the two isomers.

Let me move on at this point to our next discussant, which is Dr. Winegar.

DR. WINEGAR: In question 3, Part A, refers to the refinements in the process for determination of flux using the aerodynamic flux approach.

We have talked about the different methods for looking at flux or getting flux data. This is the third time around for most of us here. And I think there is a pretty good agreement that the aerodynamic method is probably the best way to go about doing that.

So I agree with that assessment. Some other things, though, in terms of potential enhancements to that method that might be able to fill in some of the data gaps or refinements, I guess, is more like it would be possibly to use enhanced meteorological data collection such as sonic anemometers, which have potentially a lower threshold level for determining wind speed and can get a higher frequency of turbulence data.

I know research grade sonic anemometers that can collect on the order of 200 hertz, but they are like 8,000 dollars each, which is costly. I don't know whether that's in the cards to be able to be used in the future.

But there are also reasonably priced sonic anemometers that do less but are still good in terms of the threshold, and such. It's something to consider in terms of any future studies that might be undertaken to flesh out some of the additional data that might be needed for a validation of the model.

I wasn't able to go down the road to UC Davis to retrieve this reference, but I have seen reference to an ASTM standard for validation of air dispersion models. I don't know if anybody has seen that.

I had no idea about the content of this procedure, but I will see if I can dig this up and at least incorporate it by reference into the comments. And hopefully, I'll be able to get a hard copy myself and be able to summarize maybe some of the steps.

But this is something you might want to think about in terms of some of the validation steps in going further with the model and using the flux and comparing it to some of the model values and such.

And again, I had referred to before the break, about this short-term or short distance modification to ISC from the AWMA Journal, and that has been distributed to everybody.

I can't really comment on it very much other than people have looked at that question at least recently, and I'm sure in the past to some degree also, and it looks like there might be ways to deal with some of that short distance issues, some of the limitation that are cited in regards to ISC and spatial issues there.

I had a comment, but first a question about the use of the PRZM3 model in determining flux. Could you clarify for me how that is used in SOFEA? Is that a substitute or is that an option to use to determine flux if you don't have direct data from the aerodynamic method?

I also saw reference that that's in relationship to the depth of the shank injections. So that's not clear to me how the input comes in.

DR. CRYER: To answer your question yes and no.

Yes, we did use two different deterministic models, PRZM3 and CHAIN2-D, but that was specifically to look at the incorporation depth. Is it really linear depth or is it more exponential decay? And both of those verified yes, it was more the exponential than linear. That's why that option is in SOFEA. You can either assume linear or you can assume exponential decrease with depth for that depth scaling factor.

The other thing we used it for not CHAIN2-D, but PRZM3, in this case, was for the maximum flux loss at the surface if you had a tarp.

Because we modified PRZM3 for the appropriate boundary conditions that Wang had proposed a few years ago, when you had tarps, in essence, a mass transfer resistance at the surface.

So using PRZM, in that case it came up with a value of I think 64 percent mass loss. That is an input to the model, meaning you can change it if you know of a better value or better way of estimating that. It sounds like Dr. Yates has a better method of estimating that difference.

But anyway, that's how those models were used. But yes, you can use a model like CHAIN2-D to give you hourly flux measurements and use that in lieu of field measurements if you so desire.

DR. WINEGAR: I'm not into the soil modeling thing. Are these models used extensively? Is it a pretty common application of these kind of things?

DR. CRYER: They are used very extensively by -- I know Dr. Yates' group and people that came from his group. Dr. Wang at University of Minnesota, or wherever. I don't know if that's right. So yes and no. No, they are not widespread used. They are really more research models. They are solving, governing Richard's equation and mass transport equations. They are physically based like most deterministic models. You need a lot of parameters to feed into them.

A lot of times you don't have the luxury of having measured values to put in there. So you have to make some assumptions or guesses as to what are appropriate inputs.

DR. WINEGAR: One of the things in one your papers that struck me in relation to the PRZM model is there is a parameter, script E. I don't know, there is probably some other term. But it is basically defined as a phase adjustment factor account for phase mismatch.

And I see for polyethylene film it is plus one, and for Hytibar film it is minus one, which is quite a difference in terms of it's not -- those two materials aren't that much different to make one positive and one negative. I mean, to my naive view of this.

I'm trying to put this in context of that overall model since it is a 1-D model, one dimensional model. And I'm trying to understand how that relates to reality and I see this type of a parameter that goes in there.

Can you enlighten me on that?

DR. CRYER: I can tell you what little I know about that. That was the proposal of this empirical relationship for the mass trans of resistance boundary, basically, they are like a thickness of the boundary layer at the soil surface.

That's what Dr. Yates or Dong Wang proposed. I'm not sure how they came up with that other than it probably fit data very well. Maybe they can give more details.

DR. HEERINGA: Dr. Yates, you are on the spot.

DR. YATES: It has been a while since I have looked at that paper.

We looked at two different films. We looked at Hytibar and high-density polyethylene. It seems to me as, I remember it, that the peak flux for the high density polyethylene occurred during the day and the peak flux for the Hytibar occurred during the night.

So I have a feeling that the plus, minus one has to do with the fact that one peak happened during the day and one happened at night. Although, right now without having looked at that for many years, I can't see why it would be plus one, minus one in terms of shifting the phase.

But I have a feeling that's what it is.

DR. WINEGAR: Well, without spending the rest of the day plowing through that, we'll just move on and assume that that's -- the data you show here seems to show it fits the emissions. I was trying to put that in context with everything else.

Question B, in regards to varying the flux rate probabilistically, I think that is a good thing to be able to do that, obviously. It is capability that is well worth implementing, obviously.

I would suggest some documentation on the ranges of input into the model that might be useful in terms of just guidance for potential users.

The question regarding the single monitoring study or small number of studies, I agree with what everyone else has said in regards to caveats of using. And I have said this myself, using one study to apply across the board.

And I keep coming back to the figure title summary of field studies that shows the percent of applied volatilized over days after treatment. It is on page 25 of the handout.

The Imperial California shank data, which is the bottom curve, if I'm interpreting the order of the curves correctly without seeing it in color -- let me ask you first, I kind of inferred from your comments that you don't just put a lot of weight on that, because you are saying because it didn't go out to the full extent of the rest of the other studies -- this is valid data, obviously, or you wouldn't present it. Is that a reasonable interpretation?

DR. VAN WESENBEECK: Yes. I think the quality of that study was fine. It was the first aerodynamic flux study that the company did in 1991.

And I think the only problem with that study is that it didn't carry on enough. So we don't use it, we don't ever assume just 11 percent mass loss, which is what the cumulative mass loss there was.

But I think the fact that there was a delayed flux from the surface is probably real. I think it is reasonable quality data probably just due to the soil type and the degree of soil sealing that occurred there.

DR. WINEGAR: In that event I assumed that you had confidence in that data, otherwise you wouldn't have shown it. I compare that to the third line, which is the California, Salinas shank data, which has a substantially different curve.

So I just keep looking at that and thinking this is just an example of how even within California and the same type of application method you have substantially different flux curves over time.

And so notwithstanding some of the other work that suggests that you're -- the use of that one general flux profile is useful in many situations, I look at that and think -- and think also with the general representativeness question that I cited earlier, that indeed additional flux profiles are needed in order to apply it to different locations.

And we have talked extensively in our past sessions also about the representativeness of different areas using localized meteorological data.

So my comment is that I think you would need to flesh out again with additional validation data, field data for different locations in order to be able to apply this to different regions around the country and even within a state such as California.

Can you explain any reason why these two curves really would be considered equivalent, the California, Salinas versus Imperial?

DR. VAN WESENBEECK: I don't think we're assuming really that they are equivalent. They are two different points or estimates of flux and time. And the reason the Salinas study was selected is because it gave the highest mass loss. So that was used as a worst case scenario from a modeling perspective.

DR. WINEGAR: You were trying to be conservative.

I think that's a reasonable conservative approach to take. But, again, my feeling is that each region should be represented in some way directly, instead of doing a general over conservative approach.

Question D, in regards to the multiple, linked application events.

I think, again, flexibility to be able to do more than one location is very useful and a powerful capability of the model. So I applaud you for taking the effort to do that.

I do need to clarify a little bit.

Is the general way of including multiple applications in an airshed, is that only available via a randomized type of approach like you discussed in the presentation? Or is there the possibility to put in specific locations if you have that data?

DR. CRYER: No, it randomly selects the locations, again, based on ag-capable. But you can put them in if you know roughly the area that fields are located in, for whatever reason, then you specify that section weighting in a one mile by one mile grid. So you just stick them all there.

To get to what you are talking about, that was again like the next generation of modeling where we have to use some aerial photography that's been digitized.

DR. WINEGAR: I personally have located through the use of Pesticide Use Reports approximate locations for lots of -- for one season's worth of methylbromide applications in one general area. And so it -- with a modicum of work it is possible to do that without having to resort to fancy GIS type of things.

Part E, in terms of missing data, I wasn't really able to discern whether there was any special routines within the spreadsheet to deal with missing data.

I know that since it incorporates ISC that there are procedures within ISC to deal with missing data or actually the need to have complete MET data sets, for example, in order to proceed.

Is there any part of the documentation that I missed in that regard, or can you comment in regards to, specifically, missing data, how the spreadsheet models in particular address missing data?

DR. VAN WESENBEECK: Basically, it needs to have numbers there or it won't run. So you have to -- it comes with a default set of numbers, but as you point out, the MET file needs to be complete or you will generate an error within the ISCST.

There is no real other opportunities for missing data, that I can think of, that wouldn't generate the program to crash.

DR. WINEGAR: Did I understand you correctly in regards to different options for including some of the more specialized data, for example, the agronomic data and such? As I understood you in your presentation, and correct me if I'm wrong, that type of data is considered optional, some of the land use data, that type of thing. Is that right?

DR. VAN WESENBEECK: Yes.

DR. WINEGAR: Those are my comments. Thank you.

DR. HEERINGA: Thank you very much. At this point I would like to see if any other members of the panel have comments. And we do. Mr. Gouveia, do you want to begin?

DR. GOUVEIA: I want to make two quick comments on the aerodynamic method. I would agree with Dr. Winegar about the use of sonic anemometers for -- especially for the low wind cases, it is much better anemometry.

There are also available manufactured 3-D anemometers with thermal couples that I don't know if I can name particular vendors, but there are several vendors that make these for agricultural use and flux measurements.

About the short-term analysis of the pesticide in the air, there are IR spectrometers that do measure absorption of any aerodynamic, any constituent in the air as long as you know what the absorption is in IR range.

It may or may not be appropriate for these kind of flux measurements. But things are available for a shorter time averaging and measurements of volatiles in the air.

DR. HEERINGA: Dr. Cohen.

DR. COHEN: Regarding question subpart D, regarding the multiple, linked application events, I just want to commend the model developers on doing such a comprehensive study.

Generally, when you look at these sorts of situations and in my experience looking at individual facilities, individual power plants or incinerators, often from a regulatory and from a modeling point of view we're considering just one facility.

But in reality of course the receptors are being exposed to all sorts of facilities.

And this is one of the first times I have seen somebody try to account for this and to say, well, there is going to be a lot of fields and somebody could be getting hit by plumes from all over.

So I would urge California and the EPA and other regulatory bodies to wherever possible to consider this type of approach sort of cumulative impact.

It is not whether somebody is going to be exposed to too much pesticide from one field. The question is, if you have an agricultural system where this stuff is being used everywhere, what are the exposure routes. You are asking the right question. You are trying to answer the right question. I just wanted to commend you for that.

DR. HEERINGA: Dr. Arya.

DR. ARYA: I would like to make some comments and suggestion on the aerodynamic method of determining flux.

Being a micrometeorologist myself, I, of course, am familiar with the literature, probably the best direct method of measuring flux is eddy correlation method.

That requires fast response measurements of both velocity fluctuations and concentration fluctuations better than one hertz, like sonic anemometers can measure velocity fluctuations that eddy correlation method is used for measuring heat flux, water vapor flux, because the sensor's available for measuring temperature, water vapor, humidity -- but a fast response instrument.

But it may not be practical for chemicals for which you may not have sensor which can measure very fast response concentration fluctuations.

So in the absence, of course, of that eddy correlation method, aerodynamic method of course is the best available practical method on that point of view. But typically aerodynamic method is also used for estimating heat flux, water vapor flux and so on.

But typically, averaging time implied in the usual method, the method is usually based on those equations given in the presentation, and those really are based on the so-called Monin-Obhukov theory in which you have the similarity dependent functions of dimensionless velocity gradient or dimensionless concentration gradient.

But the empirically estimated functions of those are really based on hourly average data. And so in that sense, really aerodynamic method is really applicable to hourly average, estimating hourly average fluxes while the six-hour samples become too large.

You cannot use the same similarity functions to empirically determine for six-hour averaging time.

If you are interested in six-hour long average, you determine one hour flux is and then average over six hours, rather than using the six hourly average gradients of temperature and concentrations and so on.

So I would suggest that in the use of this aerodynamic method an attempt should be made to measure actually hourly average concentration gradients even though you may not have samples every hour.

Even though samples may be five or six every day, they should be kind of hourly average rather than over six hour averages so on.

In terms of application or modified in how this flux may be modified for different temperature and so on, I would suggest instead of using temperature, stability -- fluxes, they depend strongly on the stability, rather than just temperature. Temperature is not a right measure.

The stability depends both on the temperature gradient and also the winds, wind speed. So you get very strongly unstable or stable condition under weak wind condition. That way fluxes can be very different during the daytime and nighttime in convective conditions and stable nighttime conditions.

So it is really the stability. Like in those equations there is a Cherchin (ph) number, the Cherchin number is the dimensionless measure of stability. So one can hopefully relate the flux to the Cherchin number if there is some empirical relationship how flux varies with the Cherchin number and use that kind of empirical relationship to account for that difference in the stability, rather than just the temperature.

Temperature is really, you know, the same temperature you can have stable condition during nighttime. You can have unstable conditions and fluxes will be quite different.

Those are the two comments I wanted to make.

DR. VAN WESENBEECK: Can I give a response?

DR. HEERINGA: Absolutely.

DR. VAN WESENBEECK: I would like to just address Dr. Arya's comments just now, which really echo Dr. Majewski's comments as well regarding the 6/6/12 sampling interval possibly being too coarse and missing stability periods.

A colleague just jogged my memory that we -- in some of the early studies in California we did some four-hour sampling throughout a day. So six four-hour periods and did a comparison between that and the 6/ 6/12 and came out with fairly similar estimates of aerodynamic flux for that particular study.

And also I wanted to address the ability of the model to predict near field concentrations; that's been questioned somewhat within 100 meters.

I would like to just point to the figure on Page 17 of the handout for those who have it where -- and this is a study in California, a drip flux study, where we took the aerodynamic flux profile and ran it through ISCST with the actual weather data at the site.

And we come up with, as you can see, fairly reasonable predictions, certainly within an order of magnitude, if not within 2X or 1 and a half X in most case. Sometimes it is a bit higher, sometimes a bit lower. These are for receptors at 100 and

300 feet. We do have a number of other which I haven't reported here which also indicate fairly good model predictions based on aerodynamic flux.

So I think some of that idea that we can't predict near field may be a result of older versions of ISCST where there are known over or underpredictions, I can't recall, but in general it seems to be doing a fairly good job in quite a few of our field studies.

DR. HEERINGA: Dr. Yates.

DR. YATES: I have a quick question. It kind of goes with what you are saying. This kind of rule of thumb on ISC that you can't use it within 100, is it 100 meters or feet? One hundred meters.

Is that applicable to the point source or would an aerial source be the same? Would that have the same kind of limitation to it?

DR. ARYA: The aerial source, the way it is handled is basically considered to be a bunch of point sources.

So whatever -- you know, even though the dispersion curves in this model are derived based on diffusion experiments from point sources, but line and area sources are modeled as kind of strings (ph) on the point sources, kind of divided in a line or area.

DR. YATES: So that means if you have a field that is 100 meters by 100 meters, then, theoretically, even at the boundary of the field you might get a reasonable comparison between ISC, because you have 100 meters of fetch from the upwind edge of the field.

DR. ARYA: Yes, but from the down end it is too close.

DR. YATES: Well, it is just a thought.

DR. HEERINGA: But for a 40 acre field --

DR. VAN WESENBEECK: I think also with the area source you have the lateral variability that may get damped out as well due to variations in wind within 100 meters of the edge of the field. So that may have an effect of improving the ability of the model as well.

DR. ARYA: If I can make a comment on this handout on near field dispersion model. We got this paper, this paper by -- well, they don't use ISCST. They have their own model.

But basically, the dispersion parameter, sigma Y and sigma Z described by equation 13 in near field they are based on Taylor's statistical theory, you know, 1921 paper, classical paper.

In fact, it is understood that near the source that Taylor's dispersion theory is better applicable than some of the empirical curves, which are based on that average and really were not taken very close to the source.

So this simple model here is based on this sigma Y and sigma Z are simply proportioned to the distance from the source. But again, this will be applicable to a point source only to the extent this point source is very, very, small source.

When the point source becomes a somewhat large area, then you cannot apply this too close to the source either.

DR. HEERINGA: For the record, Dr. Arya is referring to the Isakov, et. al., paper which is in Air and Waste Management volume 54. I think it is April, 2004.

Any other -- Dr. Yates.

DR. YATES: Just another comment on the scale factors.

It seems like the scale factors might be somewhat appropriate for looking at cumulative flux as a way to scale it. But for the period flux, which would be more important for acute exposures, I don't know that that sort of an approach would be the best.

But the other thing, too, in terms of looking at a scale factor where you have one for the depth of application is one of the ways that you scale the flux, it would seem like at some point it would be better to try to look at soil degradation as the scaling factor.

Because in terms of cumulative flux, if you don't have any soil degradation then you are going to get 100 percent emissions at some point in time. It is -- degradation is really what is controlling how much of the material is available to be volatilized into the air.

And the depth of application affects volatilization only by increasing residence time. So increases the time in which degradation can occur. So really, it would seem like a more appropriate way to do the scaling would be to include soil degradation into it.

So basically, it would have two factors, soil degradation and then depth of application. But to forget about the degradation altogether, your scale factor works for your model parameters that you use. But if you are going to try to apply this in a different location where those parameters are no longer appropriate, it won't give the correct kind of behavior.

But basically, in a sense, what that really is saying is that at some point it would be more accurate to move toward some more mechanistic approach for obtaining this kind of information.

DR. HEERINGA: Dr. Potter.

DR. POTTER: I just had one comment about the stochastic approach to handling flux. I think you got halfway there. And I think I have heard a number of people talk about a lot of the issues around that, looking at depth variable, application rate variable.

But the reality is we don't have a grip on the variability and the uncertainty associated with flux because of the fact that you have a single flux profile.

And so this stochastic treatment that we're looking at is kind of a sort of --maybe it is a pseudo-stochastic handling to draw upon the pseudo-validation concept that you expressed earlier.

You know, no doubt, you know, it appears that you have identified an emission curve, which is appropriately conservative.
And certainly that from a regulatory perspective is reassuring. But does it actually treat flux stochastically? I don't think so.

DR. HEERINGA: Dr. Spicer.

DR. SPICER: Yes. You did refer to this one particular figure. I believe you said it was on Page 17 of the handout where you compared the aerodynamic flux method and the back flux method. Is that the only -- you have more comparisons than that?

MR. HOUTMAN: Just to clarify, it is not a comparison of measured versus back-calculated. It is a measure versus modeled.

We measured air concentrations, and then using aerodynamic, the results of that predicted at that same point and compared the measured versus modeled.

DR. HEERINGA: There are two graphs on Page 17. I think you were referring to the top of the two which compares the model versus the actual measurements.

DR. SPICER: I'm sorry. I was actually looking at the next figure. The one that had the --

MR. HOUTMAN: That's back-calculated.

DR. SPICER: Is that the single comparison that you have between the back- calculated and the aerodynamic?

DR. VAN WESENBEECK: We have gone through the back-calculation exercise on most of our flux studies. And this is an example where it actually came out pretty nicely. There are other examples where it is not as good.

DR. SPICER: Well, I guess -- of course, I have problems with both approaches. And I guess the problem that I can end up having is exactly associated with something I believe has already been discussed.

And that is that even if you reduce the averaging times down to four hours as opposed to the 6 or 12, I think that there are meteorological conditions that you can be missing, which will affect the flux.

So the problem is that if we knew, for example, that these studies involved exactly the same amount of material, then -- and I guess for this one particular one they may under these circumstances.

But the problem is that you can just miss a lot of detail associated with the MET conditions that you are not capturing when you use these longer averaging times.

That seems to be the point regardless of whether the methods compare favorably or not.

DR. HEERINGA: Dr. Arya.

DR. ARYA: As a point of clarification with regard to this comparison, in this case you mentioned a specific monitoring location. Now, is that just downwind of a particular field apply?

DR. HEERINGA: The top figure on Page 17?

DR. VAN WESENBEECK: Yes. There were eight receptors around that field at 100 and 300 feet from each side of the field for cardinal directions.

DR. ARYA: So you are actually using the emission, measured emission at that one particular field?

DR. VAN WESENBEECK: Correct.

DR. HEERINGA: Dr. Yates.

DR. YATES: Just a point of clarification also.

You are saying that the Salinas Valley study was the worst case because the cumulative loss was the highest?

MR. HOUTMAN: Yes.

DR. YATES: It would seem to me if you are interested in acute exposure, that you might want to look at the study that has the higher period flux. And it doesn't necessarily follow that the higher cumulative loss would be the higher period -- would have the highest period flux in it.

And I notice in one of your other studies you had higher values. I can't find it now, but you had -- I think it was almost 50 percent higher, I think is what I remember, period flux, although the cumulative flux was a little bit lower.

Now, of course the timing is important too. It has to occur at a time where meteorological conditions produce a high exposure at the receptor.

But I guess the point is that to me cumulative flux may not necessarily tell you that is the worst case. It seems like it is more complicated than that.

MR. HOUTMAN: I think to reinforce what you are saying, the cumulative mass loss is important for chronic or long term exposures as a contributor to overall exposure.

But you are right. It is 24 hour or shorter periods of time in mass loss during that time interval. That's more important for acute.

DR. YATES: So most of the discussion, then, from your perspective has been for chronic exposure?

MR. HOUTMAN: Yes, one of the hallmark regulatory features of 1,3-D is its chronic exposure in risk as opposed to acute, which I think makes it different maybe than some of the other soil fumigants being evaluated.

DR. YATES: To me it is always acute.

MR. HOUTMAN: Not always.

DR. HEERINGA: I have been holding this question. I wanted to make sure, maybe stimulate a little more discussion in the Panel.

I think this issue of chronic versus acute, it certainly comes to play when we're thinking about introducing variability, sort of, potential variability into these temporal flux distributions.

And I think one question we need to ask, and maybe it would be beneficial for the EPA, and that is if it really is a chronic endpoint that we're trying to evaluate, do we want to add variability to the integrated off-gassing or do we want to add variability to the time specific points in that process? That process lasts four or five, eight days. You aren't adding variability except through the depth and the injection depths, et cetera, to the sort of cumulative off-gassing from a treatment.

You just -- when you do that, you just scale the profile up and down as I understand it. So you change the -- add variability to the integration.

Other models that have focussed on acute have literally added stochastic variability to each off-gassing or each hourly flux measurement based on some draw from the 5th to the 95th percentile. And that's been estimated different ways.

I just throw this out to the Panel as to -- first, of all this problem comes up in statistics. Do you compute the aggregates and then add the variability or do you add the variability to the components and then aggregate?

They will produce different answers in some cases.

With respect to the acute versus chronic outcomes, and we'll get to the acute in Question 7, but do individual Panel members in terms of recommendations here think that they should be migrating toward adding variability to sort of time specific flux measurements from that flux profile?

Or would it be sufficient in terms of the chronic outcomes to add variability to the integrated total flux over the release period?

DR. GOUVEIA: I just have a quick comment. It really matters how things are correlated. High flux comes during -- if high flux comes during times with great instability, well, then the higher flux value is going to be dispersed faster.

So you have confounding parameters, confounding variables. It is hard to understand before you actually do the calculations how it is going to work out.

There is probably other correlations by knowing hour by hour values, hour by hour specific values. There is probably other correlations in the mix.

DR. HEERINGA: Thank you.

I know it is late in the afternoon. I would like to ask one more question maybe to stimulate a little more discussion on this.

Clearly, we put people on all of these probabilistic exposure modeling exercises. There is not enough observational data to ever derive parameters and distributions the way we would really like to see them.

In this case, we have single studies. And often, if we have multiple studies they involve dramatically different application or soil type issues.

Clearly, what we need to do in terms of focusing this for ultimate risk assessment purposes is to capture ranges of potential variability without being overly conservative in a specific localized applications.

Are there any suggestions as to the type of approach to take there? Clearly, we would probably like to see replications 2, 3, 4 or multiple on any given method and location. That would give us some stability on sort of intra-locality variability.

But this larger issue, I don't think -- it is probably impractical to recommend any time somebody is going to move into a different area to do eight new field studies and average results.

DR. WINEGAR: In regards to your first question you posed a minutes ago about whether we vary the point by point or the cumulative, I can't really cite the whole pile of technical justifications.

But to me, it just seems like the point to point variability more represents the physicality of the situation, which, as I view modeling, the further you get away from an actual -- the physical situation, the more you are going to run into problems.

Then it just becomes a mathematical exercise.

So even though it may introduce -- I suspect it would introduce more complications to the whole scenario to deal with the point to point variability or variability of each flux point.

I believe that would be the more appropriate way to go.

DR. HEERINGA: I accept that argument.

Anybody else have thoughts on that matter? Dr. Majewski.

DR. MAJEWSKI: I may have just gotten one of those ideas. But it seems to me that you have a single flux study that you are basing your model runs on.

And then you are varying the injection depth and so you scale the flux, the cumulative flux profile, or the temperature and you add another scaling factor. And those are all important considerations.

But also, I think, ultimately, what we should strive for is to understanding what is driving that flux value. We have all the meteorological information. Have you looked at that, other than temperature like Dr. Arya said, look at the stability for that six-hour periods.

And then I don't know if you can calculate stability from CIMIS data, but look at that in the other areas and see if you can use that in your estimation or trans location of the flux data.

Does that make any sense? No? Yes?

DR. HEERINGA: Dr. Van Wesenbeeck? You are under no obligation to answer.

DR. CRYER: I just want to make a comment to that. To me, I think what we really need to do, I'm not proposing we do it, but I'm saying the scientific community, we have -- like the USDA has models that are physically based.

We have good data sets now for flux. And last time, at the last SAP I know Dr. Yates mentioned boundary condition, now they have a boundary condition that proposes the use of stability class.

Let's make use of that, and see how well it does against the data sets that we have and then you can use that to extrapolate other regions or other soil types or whatever the case may be.

In that case, then it comes down to negotiations with the regulatory bodies or whatever, what are characteristic regions in agronomic communities, what types of soils do we need to simulate.

Because we're still going to be limited to time. It takes time to do all this stuff. That was just my recommendation.

DR. WINEGAR: You mentioned -- the second question that you just posed a minute ago in regards to basically estimates of variability of the flux, overall flux measurements, was that the gist of what you were asking?

DR. HEERINGA: Yes.

DR. WINEGAR: I wanted to pose the question, perhaps, a way to arrive at that is to look again to the body of methylbromide studies, flux studies that have been done, some of which have been the aerodynamic variety.

It is kind of a cross-species kind of thing here, but it is the same technique. I just recall that I'm asked personally in a lot of the studies that I undertake, you do one or two -- collect one or two samples and everyone wants to know what is the error bounds around that.

Obviously, you don't have enough to really do decent statistics on it. But you basically are familiar with the technique and can look at other similar studies and derive some type of estimate at least.

So that might be something to consider along those lines.

DR. HEERINGA: That's a good suggestion, in fact, as I say in the absence of true observational data and multiple replications for any given compound or any given set of application methods to look at these others, we certainly don't want to substitute necessarily the sort of basic profile for the compound.

But variability at certain flux rates, you might postulate a model of variability related to the flux rate itself. And from what I have seen in the last few sessions, none (ph) of these have quite similar profiles with regard to the number of days of off-gassing and the shape of that profile. Some of them are delayed more. Some are more instantaneous.

But even just from a simulation standpoint, we'll get to that in Question 8, it might be valuable to just look at a model of the relationship between the flux rate and the variance of that rate and do some sampling, stochastic sampling, in that within time periods.

Dr. Cohen.

DR. COHEN: I think another key issue that has been touched on many times today is this 24 hour averaging question.

I think you could change your model around fairly easily to also output one hour averages or two hour averages or two-hour averages. I think -- we have heard from Mr. Dawson that there may be some concern about the acute levels.

Frankly, we probably don't know enough about the toxicology of these compounds to know should we be concerned about a one hour exposure or a two hour exposure or is it simply the long term exposure.

By putting in your model this ability to output these shorter term exposures, if we find out later on that one hour or two hours of a really high exposure can trigger some event of some sort, then we have that information.

Right now you are not -- you are sort of like not giving it to people even to consider.

DR. HEERINGA: At this point, it is late in the afternoon. We have another day or greater part of a day ahead of us tomorrow. Unless there are anymore comments that the panel members would like to offer at this point, I would like to draw the afternoon session to a close.

Before I do that, I would like to turn to our Designated Federal Official, Joe Bailey, to see if he has any comments or any follow up.

MR. BAILEY: I don't think so.

DR. HEERINGA: I think that what I would like to do is to ask the members of the panel to meet in our break-out room just to discuss plans for the preparation of our written comments on our first three responses and plans for tomorrow.

For everyone else here, we thank you for your attendance today. We'll plan to reconvene our meeting with the second day of our two-day session tomorrow morning at 8:30 a.m. in this room. Have a good evening everyone.

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[Whereupon, at 5:15 p.m., the meeting recessed.]

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